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ABSTRACT

This report of a 1974 conference on child care and public policy provides an overview of an attempt to establish a rational dialogue between public policymakers interested in the problems of child care and researchers who study the problems of child development. In Part I, a description of the historical context of issues relating to public concern about child care is presented. Possible models for effecting child care policy are discussed and emphasis is given to the development of the interactive model which is viewed as having good potential for uniting issues of child care and public policy. Part II consists of a summary of an extensive survey of federal programs assisting disadvantaged children from birth to age 9, which was used as a focus for further exploration by conference participants. The survey examines the importance of critical periods and early experience, interventions, prediction from childhood to adult characteristics and goals and standards of programs. Evaluations of early childhood intervention programs are discussed. Recommendations for child care program planning are advanced. Part III includes participants' critiques of the survey of federal programs, a brief summary of conference discussions, and a statement linking child care and public policy. (Author/RH)

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Child Care and Public Policy

A CASE STUDY

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Child Care and Public Policy

A CASE STUDY

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INTRODUCTION

In any human society, attitudes toward childhood reflect and are reflected by the social fabric of that society. As Phillip Aries emphasizes in his book, *Centuries of Childhood*, the role of childhood in the structure and organization of western society has been radically altered over the last 300 years. Pictures of children from 100 years ago serve to demonstrate the recent changes in our understanding of this developmental period. Obviously the long, dark clothes, top hats and bonnets worn by these children reflect more than a clothing style of the era. Indeed, at that time children were considered to be and therefore were treated as miniature adults.

Childhood, as it is presently conceived, is a relatively new phenomenon. No single event is more important to the history of child care than the change in our perception of the early years of human life. How long a period of development childhood is believed to be, the manner in which society chooses to regard it, and the rights and responsibilities accorded to children (including their degree of independence and relative status) are all variables which have been altered by society over time. The care and raising of the young has always been an issue of great social and psychological concern, and any discussion of childhood necessitates a dialectic of past and present considerations. Examination of the recent history of the treatment of children reveals dominant trends that require attention in the construction of public policies.

Childhood is now viewed as a unique and tender period of dependency and growth for which behavior norms for adults are nonapplicable; no longer are children thought to be but small adults. Recently, increased concern on the part of parents, teachers, and policymakers about responsibility for insuring proper development and satisfaction of needs has brought various issues of child care into the public realm. Public concern parallels the change in the conception of the significance of childhood. And this parallel development is based upon concern for who should be the final arbitrator of children's needs for the good of children.

With the change in viewing childhood has come change in assuming responsibility for that developmental period. Child labor laws represent an action in behalf of society to legislate the needs of children. This public legislation has at the same time delimited parental responsibility for the caring of children—the care of children at one time being the sole purview of parents. Thus, with childhood being regarded as a period of dependency and growth requiring nurture has come an increase in societal concern for children's well-being and a corresponding decrease in total parental responsibility.

Society is now responsible for developing public policy for child care that will in fact determine what is best for children and this will be reflected in the structure and direction of the educational programs and the size and distribution of financial allocations for these programs. Flexible, intellectually sound, and socially valuable programs need to be generated. And given the expanding intellectual inquiry into childhood and the concern acknowledged by large segments of the public, it is essential that child care policy be an outgrowth of the perceived needs of children based on the information on child care that is now available.

At present, academicians, researchers, and members of presidential commissions report findings which could affect the lives of millions of children if there were channels between these individuals and policymakers. What needs to be established is a method by which policymakers can integrate the concerns of diverse sections of the population with the information generated at universities and research institutes. Moreover, the resulting policy must then be open to public debate and, ultimately, tested in the marketplace of society—schools and social and political institutions. Questions about child care must be formulated, these questions should be handled by research when necessary, and research results, together with existing knowledge about children, must be developed into a cohesive and meaningful public policy.

The report which follows is an attempt to identify the difficulties involved in developing an acceptable public policy for child care. It is an exercise in the use of rational principles to arrive at reasonable solutions for the care of children. This report demonstrates less of a concern with actual content than with the *process* by which content can be established; thus, several models for effecting policy were evolved from a single document, *Federal Programs for Young Children: Review and Recommendations*, prepared by Dr. Sheldon White. Although the information presented here is specific—due to the fact that one group of policymakers and one group representing the scientific community concentrated their efforts on but one paper—the results of this inquiry are intended to and should be directed toward a broader perspective. This material is intended to provide the reader with one attempt at investigating and experimenting with a variety of systems for establishing child care policy.

The first part of this report is addressed to the issues and problems revealed in the linking of public policy and child care. It is a process approach that

content on the structure of the problems to be solved, the pitfalls encountered, and the conclusions reached. The second part includes the summary from White's report, which was utilized as a point of departure for the two groups, and the third part is comprised of critiques of White's report that cannot be considered as, and were not intended to be, a complete discussion of the subject matter. Within Part I, a description of the historical context of issues relating to public concern about child care is presented. Following this, possible models for effecting child care policy are discussed with particular emphasis given to the development of one model which seems to have good potential for uniting issues of child care and public policy. Part II presents the specific context area chosen for implementation of this model; obviously, other context areas may have been suitable. The six presentations of Part III include critiques of the report by distinguished colleagues in the field of child development, a brief summary of the ensuing discussions, and a statement linking child care and public policy that is an outgrowth of the conference.

In the face of rapidly altering social values, political change, and new theories for child health and growth, one must be cautious in approaching such a task as the integration of child care and public policy. Yet the needs and health of our children require that a great deal of attention be paid to these issues, and the well-being of our society depends upon such investigation. What follows then is an early attempt to bridge this gap. Far more investigation is needed but it is hoped that the formal presentation of this experience may serve to facilitate further developments.

PART I

The Linking of Child Care and Public Policy: A Process Approach

Historically, the care of children has not been considered an exalted activity. It has not required much formal education or a certain degree of wealth and has as a consequence been held in low regard. Recently, child care has taken on new importance. The impetus for this interest is due to a constellation of forces: federal and state government interest in welfare reform, educational establishment and parental concern over preschool education, recognized psychological consequences of early childhood experiences, and the changing work patterns of parents. The complex of these pressures, from an historical perspective and viewed in light of the demands of the present, create a framework that requires attention.

There now exists a demand for child care services. Unfortunately, programs responding to this need are poorly planned, lack sensible structure, and are chaotically developed and administered; the direct results are ineffectual programs. With the goal being that of optimal growth and development of children, it is necessary to combine the expertise of scholars and policymakers in the pursuit of rational, informed public policies and programs for children and their families. To this end, the problems of child care are presented here from an historical perspective, possible models for systems of child care are described, implementation of these models is explored, and future approaches are recommended.

THE HISTORICAL CONTEXT

To broaden the perspective on child care and public policy, it is important to examine the historical cycles of public concern for the care of young children. From such examination emerges two distinct trends in preschool programs—day care and nursery school. Although both of these programs followed a separate developmental course, there are some similarities. A brief historical presentation of day care and nursery school programs is provided here as background information.

Although Plato advocated that children be removed from their homes at an early age to be directed by individuals specially trained in the care and education of the young, this activity was not adopted by Western civilization. Western orientation has clearly been that of child care *within* the home. In the case of communal structure, as in hunting and gathering societies, families worked and lived together and children were raised within their common home. With the coming of the Industrial Revolution, work was no longer conducted in the home, and as a consequence, a new definition of the role of the child and of the family emerged.

There was much social concern for the children of working parents. In 1789, Count Oberlin, a Lutheran pastor in Alsace, established a center for children of mothers who worked the farms. In Paris in the 1820s and 1830s, "salles d'asiles" for the very young flourished; providing few activities of an educa-

tional nature, they nevertheless were attended by 3,600 children in 1839. Around the same time, Germany began to take care of and protect the children of working parents. These "klein kinderbewahranstalten" were founded and maintained by religious societies, women's societies, and private philanthropies. In England Robert Owen, greatly influenced by Pestalozzi, was accepting factory workers' children, as young as one year, into his nurseries although there was hostility at the time toward this type of day care arrangement. The various nomenclature of these schools—charity, industry, dane, and common day—can be traced to their divergent origins.

In histories of American education, there is scant reference to day care or nursery education. The development of these preschool programs is difficult to document completely because little information was recorded about those programs which were not a part of the public school system. However, the important influences on the development of the preschool movement in this country were industrialization, urbanization, advanced technology, poverty, the progressive education movement, and the growth of the life sciences. As a result of these societal forces, two separate, class-linked types of preschool programs emerged. On the one hand, the traditional private nursery school, designed for middle- and upper-classes, provided educational and psychological development for children from three to five years of age. On the other hand, day nursery or day care was definitely a service for the lower classes; it was designed strictly from a utilitarian standpoint—to free both partners of the working family. Although both programs served the same age range, they served different segments of the population and, thereby, established separate goals. The former was a luxury, the latter a necessity. Whereas nurseries operated only a few hours a day, day care was typically a full-day program. It was common to distinguish the two by calling the former "educational" and the latter "custodial," but in fact, there was originally little difference in the actual functioning of these programs so that the insistence on the use of such distinctions served only to thinly veil class segregation. At present these distinctions still exist.

Nursery Schools

Gesell (1943) states that nursery schools in America were first established in 1914 with initial impetus from Britain. Margaret MacMillan and Grace Owen started programs for the poor in London and Manchester, and specialists from their schools presented their work at Columbia University (Teachers College) in 1920. Around the same time, Gesell began a study of two- and three-year-olds at the Yale Psycho-Clinic, the Merrill-Palmer School was established in Detroit, and the Iowa Child Welfare Research Station as well as the New York Bureau of Experiments directed studies of two-, three-, and four-year-olds. Although some resemblance between the growth of kindergartens in the 19th

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century and nursery programs in the early 20th century is evident, there are striking differences in the effects of scientific research on these programs. Duggan, writing about the history of American education, said that "possibly no other movement in education is being so carefully developed under the guidance of research as is the study of very small children in the nursery school."[1]

Scientific interest in early childhood was generated by and resulted in new emphases in psychology, biology, physiology, and medicine and in experimental efforts in the fields of mental health and hygiene. Research efforts were supported by psychology and education departments in leading universities, by research organizations, by parents who found it increasingly difficult, under existing social conditions, to provide adequately for the education of their young children, and by individuals and associations interested in improving the educational status of day care. Research centers shared the common goal of expanding knowledge of preschool children, rich and poor. By 1936 the U. S. Office of Education reported the existence of 285 nursery schools: 77 lab (research) schools at universities, 53 philanthropic in nature, 144 private, and 11 in public schools.

One of the earliest private (nonresearch) nursery schools was organized by faculty wives at The University of Chicago in 1916.

> They felt the need, which they could not fill in their homes, of the beginnings of social contact, of group play, the chance at give and take, and the supervision at times of adults, not the children's own mothers. The mothers needed to acknowledge other children than their own, and the opportunity to test the efficacy of home training when their children joined a group.[2]

Providing group experience for their children and broadening their own experience with other children seems to be a common motivation for the creation of middle-class nursery facilities. Obviously an essential element in the design of these schools was the maintenance of close cooperation with the home and family.

Day Care

The nursery school and day care movements occurred simultaneously. Whereas nursery school was developed supposedly for educational purposes, day care was classified as a health and welfare function and thereby was considered a charitable program, largely custodial in approach. The first day care center in the United States was probably the one opened in Boston in 1838 by Mrs. Joseph Hale to provide care for the children of seamen's wives and widows. Another, founded in New York City in 1854 by the nurses at Children's

Hospital, was for working mothers' children who had been patients and was apparently purely custodial. As a result of the Civil War, some day care programs were established for children of war widows, but it was not until the Great Depression and World War II that any major program effort was undertaken by the government. Under the Federal Emergency Education Program, 3,000 facilities serving 65,000 needy children were set up in the 1930s. Under the Works Progress Administration of the New Deal, 150,000 families enrolled in the Family Life Education Program. By 1939, 300,000 children had been enrolled in 1,500 emergency centers, most of which were housed in public school buildings. However, as grave social conditions disappeared, so did most of the day care facilities. Thus in 1948, only about 10 percent of all cities in the United States were operating nursery schools or child care centers, in spite of urging on the part of educators at the "White House Conference on Children in a Democracy" (1940) that this emergency pattern of child care, maternity care, and parent education be permanently instituted. Unfortunately, the fears of the government taking over the functions of the family were strong and large-scale public support never materialized. In the 1940s and early 1950s these fears were heightened by the cold war and concern about communism.

Current Events in the Nursery and Day Care Movements

The 1950s and 1960s were a time of prosperity and security in this country, and the preschool movement again became a public issue. Many factors accounted for this renewed interest: 1) an increase in suburban living, resulting in a relatively low density of children, necessitated planned, structured play groups; 2) the decrease in the birthrate resulted in smaller families and, correspondingly, fewer age-mates within the home; 3) mechanization in the home provided individuals with more free time; 4) a greater number of women entered the labor force; 5) an increase in community aid for families, especially the poor, was provided; and 6) educators became more attuned to family needs.

Most importantly, the demand for preschool programs grew because it became apparent that the family alone cannot meet the requirements for the development and socialization of young children. As knowledge of child development increases and is further refined, the standards demanded of parents are raised. Even under ideal conditions, parents often feel unable to meet these standards.

Finally, not to be ignored is the fact that mothers do work—some for financial necessity, some for the need or desire to use skills and education, and others to escape boredom and isolation. Whereas in 1940, only one mother in 10 held a job, in 1970, for the first time, there were more women with minors holding jobs (51 percent) than those who did not. With more mothers working there are more children who need care and fewer mothers to give it.

Possibly the greatest demand for the care of young children comes from single-parent families and from families of the poor. The first large-scale public response to this expressed need was the launching of Head Start as a nationwide program in the summer of 1965. The forces that effected this innovative program still exist, and there is every indication that their efforts will be broadened. In 1973 there were six million children below the age of six with mothers in the labor force. Although the number of children in this age group is declining, the number of children with working mothers has increased. Moreover, the most rapid gain in labor force participation in recent years has been the addition of wives below the age of 35, especially those with children under six years of age. This is now an established working pattern for young mothers and clearly it must be considered and dealt with.

Ten years after the beginning of Head Start there are continuing demands for more divergent and better means of child care. Unfortunately, there is little in the way of a rational, coherent, and organized response. What is needed is consideration of past methods, gathering of information on current needs, and integration of scientific knowledge with policy administration in an effort to create an informed and viable policy.

MODELS FOR EFFECTING CHILD CARE

The present lack of communication between the scientific and policymaking communities is due to a multiplicity of factors. The policymaking community is by nature responsive—reacting to political, social, and economic pressures and needs. However, rather than initiating programs and directing public efforts through leadership and education, policymakers all too often act only when society has already responded to particular issues. Equal fault lies with the scientific community. Its commitment to theory and abstraction, rather than "real life" problems, and the use of scientific language and methodology obscure the information needed by policymakers to handle the issues creatively.

In response to the growing demand to devise a system of thoughtful and organized action for child care in this country, the Institute for Research in Human Development at Educational Testing Service (ETS) initiated a project to explore the complexities of child care and public policy. This pilot effort has been supported by the Rockefeller Family Fund and ETS. As a first step, it was necessary to establish a system of communication between those engaged in child development research and those setting policies. With the establishment of communication procedures, systematic implementation of models could ultimately be achieved. The following approaches were considered in an effort to bring the two groups together.

Position Paper Model

One means of closing the communication gap between scientist and policymaker is to commission a single, comprehensive paper on the topic of concern. Since one person would write and distribute the document, it would be completed relatively quickly. In the realm of public policy a high premium is placed on speed, and this approach could well satisfy this need. Unfortunately, this approach sacrifices breadth of coverage and interaction with colleagues; it would not encourage the kind of communication that these two distinct communities require. Therefore, a single paper may not be representative of the diversity of opinion and may not present many of the possible policy alternatives.

Group Process Model

A group communication model was considered as a means of providing the scientific and policymaking communities with open channels for the exchange of information. The scientific community could contribute to this effort by describing present levels of knowledge, how a given problem could be studied, and what areas need further investigation. The policymaking community could contribute by providing the input determining those areas that require study, analysis of what is relevant to policy formation, the needs of the community from their perspective, and what can realistically be accomplished. Thus, the communication model that was envisioned would actively involve both communities. For implementation of this general model the following specific model was developed.

Group Process Expanded. A steering committee would be established to determine specific problems of interest to the scientific community and of importance to public policy groups. Following the selection of the problem areas, position papers would be commissioned and authors chosen by the steering committee. Policymakers as well as representatives of the scientific community would attend a conference to review the completed papers and to select the best for inclusion in a book that would be distributed to a wide range of experts.

This model was inititated in the summer of 1973. The steering committee included various experts on child care and human development (see Appendix A for the list of participants). However, in the course of the first meeting in New York City, it was determined that the group process approach was not a workable model. An abundance of position papers are already available; more papers need not be commissioned. The problem is that they are not utilized by either community because they have been produced in isolation by researchers who are not aware of the needs of the policymakers. In the discussions that followed, another model was evolved.

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Interaction Model

Taking into consideration the wealth of position papers that already exist, it was decided that a committee from the scientific community should be established to act as a clearinghouse. Initially, the clearinghouse function would be to determine which of the existing position papers should be further explored. The committee would then review the selected paper for its presentation of the diversity of relevant positions and would outline the results of the work as they apply to public policy. Thus, the clearinghouse would provide a scientific forum for the discussion of relevant public policy papers.

The committee agreed that the original paper as well as individual, diverse opinions should be collected and published so that the policymaking community would have a central and relatively unbiased center to locate scientific information on policy-relevant issues. Initially, this clearinghouse function was to be the primary effort. However, in order to make this model effective, it was realized that the policymaking community would have to have the means to request the standing committee to initiate position papers in response to their specific needs. If a relevant position paper was not available, the policymaking community could ask the committee to commission such a paper. The same review process would then be initiated to examine this completed report.

The steering committee agreed on this interactive model and chose to review Sheldon White's paper, *Federal Programs for Young Children: Review and Recommendations*. White's project was supported by the Huron Institute, under contract with the Department of Health, Education and Welfare. It is an extensive survey of programs assisting children aged 0 to 9.

IMPLEMENTATION OF THE INTERACTIVE MODEL

A meeting to examine Sheldon White's report was held in the spring of 1974 at Educational Testing Service in Princeton, New Jersey. The conference was attended by researchers in child development and education, state and federal elected and appointed policymakers, nonprofit day care groups, and some representatives of private foundations and institutions (see Appendix B). Although the group was kept small enough to function as a seminar, it nevertheless was broadly representative of the influential groups involved in child care and public policy issues.

The conference was scheduled to last two and one-half days. The original agenda follows.

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Thursday, 28 March 1974

10:00 a.m.	Presentation by Sheldon White, Harvard University
Noon	Lunch
1:15 p.m.	Discussants: Carl Bereiter, Ontario Institute for Studies in Education William Kessen, Yale University Irving Sigel, Educational Testing Service Harold Stevenson, University of Michigan
7:00 p.m.	Dinner

Friday, 29 March

9:00 a.m.	Continued discussion of White's report and its relevancy to public policy
Noon	Arrival of public policy participants
	Lunch
1:30 p.m.	Overview of White's report and its criticisms: Edmund Gordon, Educational Testing Service and Columbia University
7:00 p.m.	Dinner

Saturday, 30 March

9:00 a.m.	Linking child care and public policy: Sally Allen, Education Commission of the States
1:00 p.m.	Lunch
	Review of evaluation of procedures; plans for future conferences

Those convened for the first day and a half primarily represented the scientific community. The core of this group was the steering committee that met the previous summer and also included several policymakers to express public policy needs. The intention of this group was to review this four-volume work as a council of peers—focusing on such scientific concerns as the issues of evaluation, the samples and measures used, and their validity and significance—and to then aid public policy people in its best use.

With the arrival of the other public policy administrators, the two groups discussed policy needs and alternatives. The session began with summarizing remarks by Edmund Gordon of Educational Testing Service and Columbia University, additional comments were made by White, and an open discussion followed. The next day, Sally Allen, of the Education Commission of the States, linked the issues of child care and public policy. All presentations are included in Part III.

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OUTCOMES OF THE INTERACTIVE MODEL

It should be pointed out that closure was never the intended outcome of these meetings. The issues of child care and public policy do not lend themselves to neat, simplistic answers. What was anticipated was breadth and multiplicity. Some of the issues of concern were:

- The goals of early childhood programs and their underlying values—the problem of *pluralism.*
- The alternative models of educational experience—the problem of *method.*
- The costs (both social and financial) of the various options—the problem of *resources.*
- The kinds of research findings that have been gathered to date as well as further research that may be needed—the problem of *understanding* as the basis for adaptive action programs.

White's report was used as a vehicle to discuss these issues; because it was up-to-date, broad, and available, it well satisfied the goal.

Although all participants were concerned about both child care and public policy, the group was split into "scientists" and "policymakers." This distinction was made in the belief that the scientific community first needed to subject the paper to a peer review. It was believed that this review, dealing with such issues as sample size, use of statistics, and so forth, would not be either appropriate or relevant for policymakers. In reviewing the outcome of the conference, it now appears that the split was arbitrary and that communication would have been increased had the two groups met together from the start.

In any event, the most important outcome was the interaction and communication that did result. One participant, noting a difference between the first meeting (June 1973) and the second (March 1974), stated, "Researchers seem to be considering policy issues more realistically. There's a sincere and realistic effort to listen to policy concerns. Similarly, policy people gained a new and real appreciation for the problems which researchers face."

This interaction and communication also influenced the institutions and organizations represented at the conferences. Contact between researchers and policymakers was increased, the need for more meetings was expressed, and further coordination and centralization of information was requested. It was agreed that state and local governments should be able to consult a well-informed, unbiased source when preparing for or voting on legislation effecting child care services. Such a central information source should be available to advise, to educate, and to respond to needs and should not function only when there is a crisis.

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Additional outcomes cited by participants suggest that the second meeting helped to crystalize the issues involved in providing quality services for children. It also became clear that their is a need for interdisciplinary efforts in research and planning and a need for further opportunities to exchange information and discuss concerns in order to deal with these issues.

Finally, by utilizing an existing report and basing the discussions around it, useful, practical information was obtained.

Obviously, this attempt represents just the opening sentence of a dialogue too long in coming. There were far more questions raised by the procedures, content, and style of the meetings than the number of answers provided. Yet, it was a good start according to the participants and one which they would like to see continued.

FUTURE CONSIDERATIONS

In discussing the course of future efforts to handle child care and public policy issues, two dimensions need consideration: What shall be the nature of the exchange and who should undertake this task?

Future Models

Interactive Model. One possibility is to continue to use this interactive model as the basic approach. That is, to systematically convene meetings in order to focus attention on an existing document. One such report might be that of the National Research Council, or, in the future, some of the work of the Carnegie Council on Children. Although there were certain advantages to the structure of the meetings in the past, future meetings should be established to permit the two areas to meet separately first and then to convene for plenary sessions. Because this model places a premium on interaction, the actual meetings would be rather seminar-like and small. Again, the distillation and distribution of the resulting proceedings would be crucial, and attempts for the widest circulation of the results would be necessary.

Conference Model. A second option would be the larger conference approach. Basically this approach focuses on the educational functions of such a meeting. Given its size, it would be a more passive approach than the interactive model discussed above, but it would have the effect of reaching more people faster. This might be invitational in nature and therefore selective, or an open registration may be offered with the goal of attracting a very broad and representational group of all those who might be interested. Although this second option has the advantage of reaching greater numbers of people and also of performing a service, it stresses the more passive aspects of a lecture-type situation. It is also

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possible, through this less selective model, to recapture some of the costs of the conference through registration fees while simultaneously establishing a broader base of those interested in child care and public policy.

The agenda might be the general topic of child care and public policy with several different, simultaneous sessions or workshops. Alternatively, the entire conference could focus on one specific issue. Such topics as child development programs, legislation related to child care, or industry-related child care might be suggested. This conference procedure could grow into an annual or even semi-annual event.

Public Policy Model. Another option might stress the stated needs of the policymakers. This would take the form of establishing a bureau or center to elicit specific policy needs. Knowing these, it would be possible to arrange and create seminars and conferences around the areas of substantive knowledge. For example, if a state office of child development is considering a program of infant day care, the center might create a series of lectures or papers on such issues as the costs of such programs and the numerous kinds of curricula various states have considered. Another example of a policy-geared format would be to provide a center wherein policymakers would be free to call for aid to effect impending legislation or to create new legislation.

Scientific Review Model. Still another activity might focus more on scientific and research concerns. This activity would center around a high-level clearinghouse function. Given the immense volume of existing and in-progress studies and reports in this field, a clearinghouse would be invaluable to those who are in decision-making positions (both on the policy level *and* on the implementation level). Such a clearinghouse might become the source of all up-to-date information about child development programs, research, demonstration projects, legislation, and organizations.

In addition, a clearinghouse function might be that of producing scientific review of existing public policy papers. This review would serve the function of a quality and diversity check on papers considered relevant for public policy concerns. Care should be taken to insure the proper review procedure for position papers. Such an activity was envisioned in the original interactive model that was proposed. Without a public policy orientation the scientific review becomes just half the story and may be incomplete from a policy perspective.

Organizing Role

All four of these models could be elaborated further, especially with emphasis on who should provide these services. It was the general consensus that a nongovernmental, nonprofit group would be best suited to perform any or all of the various outlined functions that are needed. While university-based organ-

izers have the requisite independence and objectivity, their major commitment is to teaching, which would not permit them to devote the amount of time that is necessary. Moreover, continuity might be difficult to achieve within the university setting. It seems that these activities might best be placed in a non-profit institution having the human and capital resources necessary to make this area one of prime concern.

In Conclusion

The enthusiasm and interest generated by these initial meetings on child care and public policy are clear indicators of the continued need for communication and interaction in this field. There is also reason to believe that the introduction of such bills as the Mondale-Javits Child and Family Services Bill will be topics of much discussion in this decade. This is not a passing fad of the 1960s. The introduction of this bill guarantees that these issues will be part of our national agenda, and, in some way, it must be a part of our agenda as well.

Footnotes

[1]Duggan, S. *A Student's Textbook in the History of Education*, p. 327.

[2]Bass, A. A cooperative nursery school. *Survey*. November 1925. LV, p. 217.

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Appendix A
Steering Committee
Child Care and Public Policy Conference
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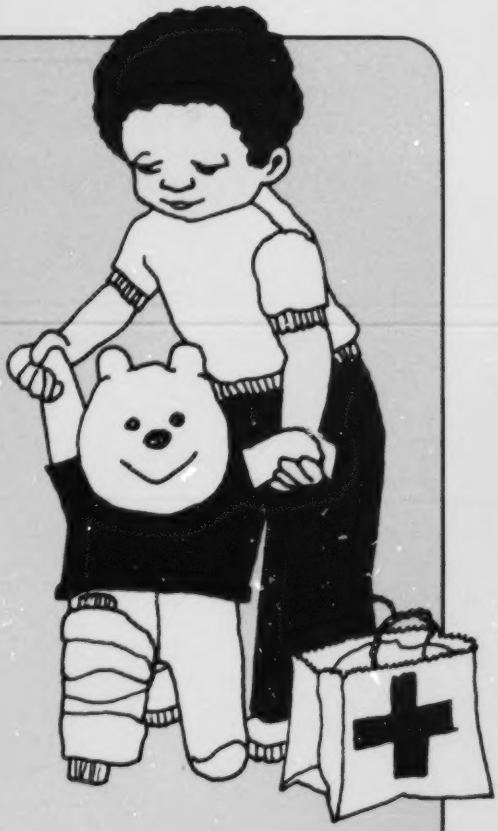
PART II



Federal Programs for Young Children: An Interactive Model

In this section is a summary, which was prepared by Dr. Sheldon White and his associates at Harvard University, of the report written for the Department of Health, Education and Welfare, the Office of the Assistant Secretary for Planning and Evaluation. The summary is presented here in full so that the reader can better understand Part III—the formal critiques, summary, and statement made by distinguished scholars at the Child Care and Public Policy Conference.

Although federal programs are only a small part of all public programs on behalf of children, it is an area of major importance because of its influential role and responsibility in stating national goals, standards, and priorities. The study relies primarily on analysis of formal data in an attempt to move from informal to formal evidence in the evaluation of programs for children. The discussions that are presented in Part III shed some light on the successes and failures of this approach.

The actual report is in three parts. "Goals and Standards of Public Programs for Children" focuses on the following areas: purpose and design of the report, the evolution of public programs for children, critical periods and early experience prediction from childhood characteristics to adult characteristics, reviews of evaluation in four major areas of intervention, and health care. "Review of Evaluation Data for Federally Sponsored Projects for Children" examines data in five particular categories: early elementary education, preschool intervention, day care, family intervention, and health care. "Recommendations for Federal Program Planning" is directed toward three major areas: recommendations regarding preschool and day care programs, recommendations for emphases in programs for children, and recommendations regarding future analysis. All sections of the report are touched on in the summary that follows.

Federal Programs for Young Children: Review and Recommendations

This study reviewed existing data about child development and evaluations of programs for children as a basis for recommendations for federal program planning. It dealt with disadvantaged children aged 0 - 9 and the federal programs now set forth for their assistance. The study was an extended examination of two recent premises about government action on behalf of such children—first, that program planning can be guided by child development data and program evaluation data and, second, that goals for such programs can and should shift from crisis intervention toward a broader concern for the enhancement of child development.

At the heart of the issue to be addressed is the question of what childhood disadvantage is. But "disadvantage" is a complex and surprisingly subtle notion and required several steps in thinking to come to a definition.

The term "disadvantage" is widely used—in public discussion, in scientific writings, in congressional testimony—and a first step in understanding it comes when one recognizes that it is applied to many sorts of children, many negative circumstances, many programs of childhood. When solutions for disadvantage are proposed, this multiplicity at times seems to be stereotyped, as when the problems of the disadvantaged child are identified with those of the average black child or the average poor child. At other times this multiplicity seems to be preserved but in an unclear way, as when it is argued that programs or services for children should be comprehensive but without any indication of the kind of diversity implied in comprehensiveness.

Examining the various usages of the term, five standards of disadvantage can be identified, each of which is necessary if one would include all of the children identified as disadvantaged in various public discussions. These standards are: (1) income, (2) ethnicity, (3) home environment-social class, (4) crisis, and (5) equity. They are correlated standards, in the sense that a child identified under one will generally also be identified under others. But not all children, nor all relevant problems, will be included unless all the standards are applied.

However, not all children deviating from the norm with respect to such standards will be defined as disadvantaged. Only some will. What kind or degree of deviation from the norm according to these standards will qualify a child as disadvantaged? Generally, it seems that the degree of deviation will be deemed serious if it is seen as jeopardizing certain "public purposes" with regard to the upbringing of the children. A person takes an interest in every aspect of the well-being of his own child, but there are only a selected number of issues that will bring about a concern for the upbringing of the generality of

children in one's society. The public purposes reflect these issues. They are to some extent issues of the present and future well-being of the society, and they are to some extent issues of compassion extended to children as well as to others in the society. Historically, public intervention in child care and upbringing seems to have originated in these public purposes and, at root, the notion of "disadvantage" appears to arise from them. They are:

1. To see to it that a child learns and develops in such a way that he can take up some reasonable vocational or economic place in society.
2. To provide for "political socialization" in the early years: to assure that normative standards of American life, patriotism, and a conception of self as related to society, are instilled in the child as he grows up.
3. To regulate the labor market by (a) restricting the use of children as laborers and (b) providing supervision for the child to release the parents for employment.
4. To provide help for the child in certain kinds of crisis situations—on a compassionate basis or on the basis of calculating ultimate benefit to society, or both.

The disadvantaged child is, in general, that child for whom the expression of these public purposes is inadequate—for whom there does not seem to be a clear path to some economic place in society, who grows up feeling excluded rather than included in American society, or who is at risk because of a variety of family crises, handicaps, or health factors.

The value of a conception of public purposes seems to lie in the understanding it offers that public intervention on behalf of children has been, and probably is still, guided by certain constant and definable motives. If one examines the history of public programs for children, one sees these motives in existence from the colonial period forward. As American society has changed over the years the pressures of factors like industrialization, immigration, and urbanization have put strains upon traditional roles and institutions. Older arrangements for the care and upbringing of children have felt these strains. In addition, social institutions having implications for the upbringing of children have changed—patterns of housing and community life, the amounts and kinds of adult employment available, the role of women in the labor market. When such changes have brought some children in jeopardy—a jeopardy defined as a shortcoming in the expression of the public purposes for them—such children have been regarded as a subject for public concern.

It seems useful to imagine a system of implicit "contracts" governing the divisions of labor among those who take care of children. There is such a contract between the middle class family and the public schools today. The family will teach the child English. It will give the child training in manners and mores; it will give the child an expected amount and diversity of intellec-

tual experiences; it will have schooling in mind and it will be concerned in diverse ways to make his expected entrance into school successful. Schools depend upon some degree of such family activity. There is another such contract between the middle class family and the pediatrician, by which a cooperative pattern of activity concerned with the child's health is established. An elaborated system of such "contracts" exists in our society, providing for the sharing of child care and upbringing among family, schools, and various professionals and providing, further, for social patterns of support stemming from private charities and the several levels of government. At the heart of this system of contracts is the family. Institutions dealing with the "normal" or "average" child are keyed to expected "normal" or "average" family performance. Usually, although not always, the child who is seen as disadvantaged is that child for whom the family cannot or does not provide a "normal" or "average" amount of care and upbringing. As might be expected in any system governed by contracts, however, this kind of problem is open to some dispute. It is at times argumentatively assigned as the family's weakness, as a matter of weakness in the child, or as a matter of weakness in the social institutions that surround the child and the family.

It seemed worthwhile during the course of the study to try to examine the historical expression of the public purposes governing childhood, as manifested in changes in the various social contracts governing the upbringing of children in American history. There has been a historical rise in governmental involvement in the care and upbringing of children, and a brief glance at history suggests the kinds of circumstances that has brought about that rise:

1. *Preparation of children to assume adult economic roles.* There has been a shift from family and private responsibility for such preparation to public auspices and, since 1850, there has been a steady trend toward more publicly sponsored schooling for more children.

   The trend toward schooling has been supported by public beliefs that the public schools would: (a) adjust the child to the work roles of an industrial society; (b) clear the streets of unemployed youth; and (c) by teaching skills, make all children economically productive.

   Recent debate has focused on the extent of the vocational function of the schools, and the extent of responsibility to be assigned to schools if they do have this function. Do schools now represent a fully rational investment in vocational development of individuals or economic development of the society? Will increased investments in the schools increase their utility in these regards? Can the federal government, through schools and the principle of "equality of educational opportunity," insure equal opportunity for all citizens to participate in all economic roles?

2. *Assimilation of children into a socially cohesive nation.* With the coming of public schooling, there has been a persistent belief in the use of the schools to "Americanize" children.

Arguments for this function have been historically prominent whenever large waves of immigrants have come into American society, particularly when their foreignness or ignorance of American traditions have been perceived as socially disruptive. Since the 1960s, when the first vigorous attempts were made to include Blacks, Indians, and Spanish-speakers in the vision of a unified American culture, there has been debate about the function of the schools in assimilating these groups.

The assimilative function of public schools is real and rational. Public funds are reasonably used to recreate and strengthen an American social cohesion. Current events demonstrate the problems of maintaining unity in nonhomogeneous societies. Schools are one instrument of a more complex assimilative solution in the future, either the traditional "melting pot" solution or that more complex solution envisioned in the notion of a "pluralistic" society.

3. *Partial regulation of the labor market.* One reason for the rise of public schooling has undoubtedly been the increasing social need for a more educated labor force, but there have been other factors connected with the labor market as well. Historically, the rise of the schools has been tied to concern for the restriction of the use of children in labor. When children moved into the public schools en masse this corrected conditions of abuse in labor that had been a matter of social concern for some decades preceding. Their move into the schools may also have reflected a marked lessening of the need for children in the labor market. One can ask whether the trend in this century for more and more children to seek more and more school reflects simply the pull of the schools or, as much, the push of a shrinking supply of jobs.

   Of some importance, also, has been the utility of the schools in freeing the parents for labor by providing some amount of day care. Public policy regarding children has recently been influenced by demands for extensions of publicly supported day care. Since 1967, attempts have been made under WIN child care and state and local day care services to encourage mothers of families on AFDC to get jobs. There is less consensus on the government's role in supporting day care for other income groups.

4. *Public care for children at risk.* "At risk" categories of children have always been subjects of social concern and responsibility. Many kinds of children at risk have been treated by public action for centuries: the physically handicapped; the diseased; the emotionally disturbed; the mentally retarded; orphans; children of disabled or absent parents; the neglected or abused.

   Historical trends in care for children at risk have lead to: (a) extensions of services to more children; (b) enlargements of the proportion of children defined as "at risk"; (c) increases in differentiated categorical services; (d) a progressive transfer from private to local, and then to state and federal responsibility; and (e) the relatively more rapid development of these trends in urban than in rural areas.

THE "DISADVANTAGED" CHILD IN THE 1960s

During the 1960s there began a new forward movement in programs for children and in public concern for children. It seems fair to say that for many people, this seemed like a new impetus not connected with a past and a history. In fact there was much in the movement that seemed directly related to the issues of the 1900s, the period that had produced the first White House Conference on Children and Youth and had led to the creation of the Children's Bureau; and the issues of the 1930s, the time when Social Security and AFDC had come into being. What was most prominent in public discussions and what seemed new was the issue of the Blacks and the Poor, for a time treated as virtually synonymous with one another. What also seemed new—and, in a sense, was new—was the heavy use of scientific data of childhood in justifying programs of action for Black-Poor children. Closely related to the seeming promise of such data was also the move, for the first time, to formally provide for the collection of data about the children in the new programs created at that time to allow for official evaluations of program effectiveness.

As is usually the case for political programs, the initiatives towards programs for children rested on compromises among several conceptions about the central issues to be addressed. These several conceptions are found blended in the thrusts of the programs. For example, some of the following issues are addressed by recent activities on behalf of children:

1. *The issue of community action and community control.* Some have seen children's programs as part of a complex of activities designed to bring about political and institutional participation on the part of the poor. They have felt that a central goal for such programs is to bring about some degree of power on the part of the poor to force institutional responsiveness. Thus, Head Start was originally designated as a Community Action Program and some originally argued that a prime purpose of the program was to draw parents into community action. Follow Through and the Elementary and Secondary Education Act programs have placed emphasis on parent advisory groups and parent participation. The recent report of the Joint Commission on the Mental Health of Children recommended a nationwide system of Advocacy for children, a theme picked up by the recent White House Conference on Children, and embodied in the recent fundings of demonstration Advocacy and 4 C's projects.

2. *The issue of service coordination and comprehensiveness.* Some have seen the newer programs as extensions of the services offered under the aegis of the series of older programs for children. They have been concerned to address a contemporary problem in the administration of services under the older programs, their redundancy and discoordination of services and the

difficulty in achieving comprehensive services for children. Thus, Title IV B of the 1967 Social Security Amendments provided for support of comprehensive services on behalf of children. Head Start and Follow Through were mandated as comprehensive service programs. The goal of the Parent-Child Centers has been to achieve comprehensiveness of service.

3. *The issue of family support.* Some have seen the contemporary family as in distress because of a complex of contemporary social forces. The early wave of programs of disadvantaged children was concerned to bring children intellectual, social, and emotional stimulation that might not be available under difficult family circumstances. The issue of early education and early stimulation through resources outside the family has been the most commonly understood central issue of the Head Start Program. More recently, emphasis has shifted towards the use of programmatic resources for relief of the family in the several recent initiatives towards the provision of a national day care system.

4. *The issue of child development.* The central issue in all programs for children is, in a general sense, the betterment of children's welfare and the fostering of child development. The recent programs have emphasized some rather specific assumptions about children's development, and have been concerned to make a direct effort to intervene in their development. It has been argued that poor children or, more generally, all children living under conditions of disadvantage, may suffer serious environmental deprivations or deficits in their early years. These early circumstances may be critical for the child's development because it may be difficult or impossible to correct for them in later life. And these early circumstances may be critical in determining subsequent serious trouble for the individual and for society when the child reaches adulthood. Viewing the problems of disadvantaged children in this way, it becomes quite important to try to provide for proper early environment of the child.

   Various kinds of scientific data have been invoked to support these arguments for the special importance of early intervention. Of particular significance has been a special, restricted version of this argument. There is a notion that human IQ is plastic in the early years, modifiable by a stimulating environment, and this notion has led some to envision the purposes of early childhood programs largely as directed at IQ modification. This has given rise, in turn, to a counter argument holding that the IQ is largely determined by heredity and, therefore, there is not much hope for early intervention programs.

The four kinds of issues just discussed have determined much of the design and discussion about recent government programs on behalf of disadvantaged children. Our concern, in this analysis, is largely directed toward the last-named issue, the issue of child development data that are now seen as defining

critical deficits or deprivations of disadvantaged children. We are concerned with our present ability to determine program effectiveness through such determinations.

Critical Periods and Early Experience

Many of the educational intervention programs currently being implemented for children under six years of age are based on beliefs about the criticalness of human early experience for further development and later functioning. How well founded are these beliefs?

The concept of critical period, in general, refers to a time span during which specific experiences can irreversibly alter later functioning. As used in embryology, the critical period refers to the time during which an insult may have an irreversible effect on the later form and functioning of an organ or organ system. Clear evidence exists for critical periods in the embryological development of both animals and humans. By analogy to the fields of embryology and ethology, some psychologists have argued that there may be periods in human postnatal development during which certain environmental stimuli exert their maximum effect on some physical or behavioral charactersitics of the organism.

Present data concerning sensitive periods in behavior development come from a large body of early experience studies with animals and a limited body of evidence from humans.

Animal studies have explored the effects of (1) deprivation of normal environmental experiences, (2) enrichment of the environment, and (3) stress. In general, these studies support the notion of some form of sensitive period for socioemotional and some forms of physiological development. For intellectual development, the evidence is less clear. Furthermore, the precise amount of stimulation necessary to produce effects is not easily determined, and often effects have been found only after animals have been placed in extreme, out-of-the-ordinary, environments.

Most human studies have focused on the effects of deprivation, i.e., of institutionalization and mother separation. It is clearly not possible to do direct manipulation of early human experience, and such information as we have comes from infants in unusual circumstances. Little is known with any certainty about the effects of enrichment and of early stress.

Studies of maternal deprivation and separation typically report only short-term effects, and it has been difficult to determine with accuracy such important probably modifying variables as the conditions surrounding the separation and the quality of the pre-separation environment. For the most part, the data are inconclusive except for rather extreme situations. Children institutionalized from an early age show retardation in language, motor, and socioemotional development. Where studied, few long-term effects have been found unless the deprivation itself has occurred over a long period of time. Studies of mother separation suggest that the immediate effects of separation from the mother

and mother figure are greatest between the ages of six months and three years. This period is argued by some to be a sensitive period for the formation of human attachments and later social development.

There had been few experimental attempts to enrich a child's environment outside of institutions until the mid-1960s. The classic studies of Skeels and his colleagues and B. White's recent infancy research are frequently cited as evidence of plasticity in human development. Because their subjects were different from the norm in many ways, however, the studies do not indicate the possible plasticity of the average child within the range of average environments.

At the present time, it is clear from the animal and human data that both extreme (e.g., continued conditions of isolation, deprivation, or enrichment) and a few seemingly minor (e.g., stress immediately after birth) alternations can have immediate, if not long-term, developmental consequences. (1) Animal research has indicated that physiological changes in brain weight and chemistry may result from special environments (at both early and later ages); (2) changes in perceptual and cognitive functioning occur as a result of changes in brain physiology; (3) early experiences with other members of the species, peers and parents, may affect later social and sexual development in humans and other animals; (4) early perceptual experiences may be crucial to the normal development of sensory systems and may be dependent upon motor experiences for input; (5) early stress seems to affect the development of stress systems (hormonal and neurological); and (6) the more severe in intensity and length the deprivation, the more domains seem to be affected.

In summary, if we take the comparatively rich data from animals, it can be said that we know definitely that early experience can have significant determinative effects on later development. However, we know this through extreme studies. We do not know how well buffered the animals' systems are against more minor variations or, if they are not, how significant the effects of those variations might be. We do not have a systematic understanding of how early experiences of animals act and interact. Our data for humans are comparatively quite sparse. Although theorists like Freud have strongly argued for the critical importance of early human development, subsequent data have not elucidated the arguments to the point where we can use them as bases for specified interventions. Our data do not establish the existence of human sensitive or critical periods in early life—nor do the data exclude their existence. The issues are not settled. We cannot yet project on a scientific basis critical events and circumstances that should be the target of early childhood intervention.

Modifiability of IQ. The standing alternative to the sensitive period argument for intervention is a hereditarian argument against intervention. Recent public debate has focused on the possible modification of IQ through early childhood programs, especially programs for Black children, and has revived the argument that lower IQ scores may be unmodifiable. This debate radically oversimplifies the issues for child development programs, and finds unwarranted conclusiveness in the research literature on the subject.

Although it is likely that the heredity versus environment argument will continue to be debated with regard to IQ, the data do not support extreme positions. What we know is that (1) the IQ among individuals of differing socioeconomic status is likely to reflect, in part, hereditary factors; (2) racial differences in IQ could conceivably reflect genetic factors, but one cannot justifiably use indices of heritability based on white data to make judgments about members of a group who through social discrimination are crowded towards lower socioeconomic status; and (3) no scientific data preclude the possibility of a permanent elevation of IQ through environmental manipulation—nor do any scientific data conclusively prove that it can be done.

It might be added that the heated debates about the inheritance of IQ reflect some large and unwarranted assumptions about the meaningfulness of this index number. Over this century, IQ testing has become popular in American education because, for better or worse, such testing has allowed us to systematize and objectify some difficult human and social decisions. But it is not clear that the IQ test is a definitive measure of human intelligence or capacity or ability in the sense or senses in which they are commonly understood. In a rather famous scientific definition, the IQ test restricts itself to determining "the intelligence that the intelligence test tests." Nor is it clear that intelligence as estimated in any way should be regarded as the complete basis of human merit. There have been discussions recently in which questions about the heritability of IQ have been linked with the issue of whether American society is or is becoming a meritocracy. It would be nice to settle once and for all the question of whether human destiny is decided by fate or circumstance. However, the apportionment of hereditary versus environmental variance in IQ tests will not settle this question. IQ testing has some demonstrated utility in a statistical predictive sense. But it would be unfair and unreasonable to project serious social decisions for or against intervention on the basis of the presently known properties of the IQ index.

Prediction from Childhood to Adult Characteristics

In order to intervene successfully in childhood to enhance adult functioning, it would be useful to know the relationships between childhood circumstances and adult status. Life history studies have provided some information on the relation between events and circumstances in childhood and those in later life. The life history studies reviewed were: (1) studies assessing the stability of human characteristics over time, (2) follow-up studies of variables in childhood thought to significantly influence adult life, and (3) studies which start with various adult characteristics and "follow back" to childhood to attempt to determine their antecedents.

Stability of Characteristics over Time. Some efforts have been made to test children repeatedly on the same characteristic as they grow up, in order to try to estimate the stability or instability of the characteristic during human development. Unfortunately, such studies are extremely time-consuming and extremely rare. The few studies we have suggested are the following:

- Excluding obvious conditions of retardation, scores of child development during the first year of life bear little predictive relationship to later IQ. However, the correlation with IQ at age 16 is around .92. These data suggest that human IQ stabilizes at around adolescence. Unfortunately, there are technical features of present-day IQ tests that tend to force the appearance of such stabilization. Those technical features were built in when some data convinced the early testmakers that there is an asymptote of human mental ability of adolescence. It is not certain that that assumption would be reconfirmed today.
- School achievement also is somewhat stable over age. The direction of consistent changes in achievement appear to be related to the environment of the child.
- Very little is known about the stability of human personality characteristics through development. It is difficult to be certain that the same human trait is being assessed at different ages. Currently, two characteristics have showed some correlation from the preschool years to maturity—aggressiveness in males and dependence/passivity in females.

Predictive Factors. Literature concerning several commonly studied predictors was reveiwed—birth difficulties, single parent families, child abuse and neglect, and mental retardation.

- There is a relationship between premature birth and later lower IQ scores, lower school achievement, deviant behavior, and physical defects. Poorer children are more likely to show the constellation of deficits than their peers, and there is an argument that favorable family circumstances may act to moderate or counteract the developmental risks coming from prematurity. Similar effects of anoxia or perinatal stress on later IQ, personality, and achievement are suggested, but the findings here are less consistent. The relationship seems to depend a great deal on the severity of stress and the time of measurement of detrimental consequences. Some evidence indicates that both the incidence of birth difficulty and its tendency to bring about later negative effects are associated with family characteristics related to low socioeconomic status.
- It has been argued that a father's absence from a home bears a causal relationship to male children's delinquency, low intellectual ability and achievement, and poor psychological and social adjustment. The evidence supporting this

argument is weak. At this time one can conclude only that the impact of father absence seems to depend as much on the family conditions before and after separation as on the physical presence or absence of the father. It may be that here, as in the case of prematurity, other favorable family circumstances may moderate or counteract the possible negative effects of father absence.

- Longitudinal research on abuse, neglect, and undernourishment as a result of maternal deprivation is quite limited. The existing research suggests a relatively high proportion of serious negative effects of continual abuse, such as brain injury, mental retardation, permanent physical injury and emotional problems.

- Studies conducted during the early years of this century indicated that familial mental retardates often adjusted well in the community, found unskilled though marginal employment, and in general had fairly positive life chances. Recent studies have shown the mildly retarded to have become distinctly more marginal in terms of adult income and social class. The social adjustment of a low IQ individual, then and now, depends on a number of social and personal qualities. IQ alone is not determinative. In general, however, it appears that as American social frame has changed, as there has come to be less place for unskilled labor, the predictive consequence of early familial retardation have become more predictable and more negative.

Adolescent or Adult Variables. Four outcome statuses were considered: juvenile delinquency, school failure, income, and occupational success.

- Almost all known or imaginable adversity and stress factors of early childhood have been proposed as direct sources of delinquency. Repeated attempts to pin down a single controlling variable—IQ, SES, cultural difference, family pattern—have not been persuasive thus far. One problem may be that the incidence and distribution of delinquency is distorted in arrest-record data. Another may be that delinquency is so heterogeneous an outcome status that it is unreasonable to trace that status back to anything but gross, probabilistic, interacting sources.

- Although the dropout rate in schools is declining, a significant number of students continue to leave school before high school graduation. Efforts to predict dropouts have concentrated on early school difficulties, personality characteristics, and environmental factors. This line of studies cannot yet indicate which of a large number of potential dropouts will actually leave school, nor have effective preventive projects been developed.

- Income and occupational success have been related to region of birth, race, family size and stability, and parent education and income, but the complexities of the interactions make it virtually impossible to identify, except at the extremes, determinants of variations in adult income or educational achievement.

Summary. The literature reviewed does not point to powerful single variables in early childhood that can be manipulated to positively affect large segments of the population of children. We do not know enough about human development to identify precisely the developmental antecedents of particular adult characteristics and, in fact, the whole notion of identifying simple or specific factors in early childhood may be deeply misleading. The one generalization emerging again and again is that a number of factors and their interactions must be considered simultaneously.

For example, two of the potentially negative factors in childhood, prematurity and single parent family, are said to interact with SES. Low SES can be conceived of as a generic term which refers to a variety of potentially negative influences on development, encompassing within its scope low income, poor living conditions, delinquent peer groups, family stress, health risks, low expectations, etc. We do not know what to make of an observed influence of "SES." The multiplicity of the potentially negative influences would not only render one variable prediction virtually impossible, but also lead us to expect that the success of an interjected one-variable "positive influence" would be minimal.

Arguments have been made for decades that social phenomena—e.g., the socialization of the child—must be studied as resultants of fields of dynamically interacting factors. Unfortunately we do not as yet know how to model network causation in any rigorous way.

In the case of mental retardation, and also in the other cases, it is clear that many problems of children must be viewed from perspectives beyond child development. It is not axiomatic that one helps the child retardate by services directed at him or his family. Instead, this problem and others should be attacked through attempts to change the social game, or the interface between the social frame and the individual, as well as through attempts to "fix" the individual.

GOALS AND STANDARDS OF PROGRAMS

Education and Child Development Programs

The shortcomings discussed in the section just preceding qualify our ability to set forth positive goals and evaluative standards for programs for children. If we knew the critical factors in early development, and if we knew the connections between early childhood experiences and adult outcomes, we could use that knowledge to specify specific targets of intervention and specific criteria by which intervention might be judged. Our formal, codified knowledge is not adequate for these tasks. We mount our programs on behalf of children using

ordinary human judgment and intuition. We take guidance from data when there is such guidance. We get along without formal data for that large number of questions for which formal data do not provide answers.

Although it has not been well recognized, a state of affairs very much akin must hold in the evaluation of programs. Over the past decade, there has been a strong trend towards project and program evaluation using objective scientific techniques. However, no evaluation study can have a credibility that exceeds the credibility of the indices and measurements available for it. When we evaluate, we must inevitably make some scientific judgment of the status of a child, his family, or his circumstances. There is a "state of the art" in psychological and sociological assessment. No evaluation can exceed in validity or credibility that offered by this "state of the art."

Over the last decade or so, the time over which numbers of formal evaluation studies have been mounted, evaluation studies have used existing testing or observational techniques or relatively straightforward elaborations of them. Serious problems exist in providing measurements and indices to gauge the extent to which programs are accomplishing their generally understood purposes. The problems are these:

- Generally, the available psychological tests seem most adequate and trustworthy when measuring the traditional cognitive issues of IQ and school achievement. Tests to measure children's motivation, attitudes, or personality characteristics (usually called "noncognitive measures" or "social and affective measures") are of uncertain validity. Furthermore, it is difficult to interpret the meaning as well as the short- and long-term importance of changes obtained on such indices.

- There are important limitations to even the most widely accepted of our measuring instruments, the IQ and achievement tests. They give little useful detail about the programs measured by the tests. Different tests are only grossly interchangeable with one another. Selection of any particular tests involves the acceptance of often unknown biases favoring some kinds of program activities over others. Generally speaking, existing tests favor programs that directly or indirectly teach the test.

- Existing tests provide an estimate of individual characteristics. If the issue being addressed by a program is an individual or psychological problem, then testing might find positive benefits. But underlying most public programs for children are purposes that are partly psychological, partly sociological. Testing to date is weak in establishing social or distributional effects.

Existing instruments are, relatively, most adequate for assessing effects on children of early school experiences; next most adequate in assessing preschool effects; and least well developed for the assessment of day care effects in the 0 - 3 age range.

Although there is much interest in noncognitive measures, a review of a large number of noncognitive measures shows all presently deficient on basic issues of norming, reliability, and validity. This is of some importance because many project directors of schools and preschools place their faith and their emphasis on goals that are noncognitive.

The most promising approaches to index development right now reflect (1) an emphasis on process rather than product—e.g., the "cognitive style" tests; (2) criterion-referenced rather than norm-referenced assessment; and (3) an emphasis on observation rather than testing. With reference to the emphasis on observation, one present scheme of school classroom observation—the *Indicators of Quality* instrument—looks particularly interesting. It is built around professional educators' judgments about what makes up a good classroom environment. It is sensitive (as the achievement test often is not) to factors that educators believe influence school quality. However, its predictive power for the child's later efficacy beyond the classroom has not yet been determined.

Many of the limitations of existing tests, particularly limitations on noncognitive testing, reflect limitations of basic theory and conception of the underlying human functions. It is unlikely that we will be able to arrive at credible program indices of success simply by concentrating on test or index development.

Our current ability to assess the effectiveness of intervention for children's education and development is, at best, limited and argumentative. Program studies can provide useful data—at times, highly significant data—if they are carefully and prudently interpreted with an eye to the meaningfulness of the indices they employ. But their utility is uneven. There has been an argument that program management will be able to make go/no-go decisions through scientific program evaluations. It seems most likely that for some time to come we will have to evaluate the programs as we now must mount them. As mentioned before, we will use much ordinary human judgment and intuition; we will take guidance from data when there is such guidance; and we will get along without formal data for that large number of questions for which formal data do not provide an answer.

Family Intervention Programs

Our ability to measure the effects of family intervention programs is even more limited than our ability to create direct indices of child development. Program goals in family intervention involve either broad attempts to reduce family stress through family therapy or social referral, or attempts to educate and train parents in specific areas of child development—for example, with respect to the nutritional needs of their children or the danger of plumbism.

40 Four types of evaluation have been used generally to assess the effect of family

intervention programs: demographic measures, direct observations, rating schedules, and parent attitude questionnaires.

Demographic measures are used to gauge changes in employment, indebtedness, health status, or use of community resources like family planning services. Although easy to gather from census and labor statistics, they are often unreliable. Direct observations of behavioral changes in children or parents are common in evaluating behavior modification intervention. Their weaknesses are that the change in behavior may be superficial or not generalizable to other situations and they are expensive to construct and implement. Rating schedules combine demographic data and interviews with family members; their function is primarily to diagnose the difficulties the family is having. Parent attitude questionnaires are of dubious validity, in large part because the reported change in attitude is not necessarily reflected as a change in behavior. More adequate evaluation of family intervention programs will depend in part on better theories of family functioning and partly on a closer match between program goals and the type of evaluation used.

Health

There is clear evidence that significant health risks to children and particularly to poor children persist despite the general improvements in American health over the last century. Some of these risks clearly satisfy the criterion of a "critical period" for intervention in early childhood. That is, the problems can only be solved by intervention during pregnancy or early in childhood. The problems, if not corrected, lead to significantly reduced life chances for the child; even available, compensatory "cures" are not as effective as prevention of these conditions.

Detailed differences in incidence of such health problems among the poor and non-poor are not known. However, certain known health differences stand as indices for the constellation of health problems, and for the adequacy of the delivery of health services to the child. These indices demonstrate the higher health risks to the poor child in the following ways:

- Infant mortality rates differ according to ethnicity, socioeconomic status, and parents' education.
- Poor maternal factors, associated with poverty, are known to be associated with risks to the child. These include the mother's age, the spacing of her children, her overall health (present and past), and her proneness to prenatal complications (prematurity, toxemia, etc.).
- Poverty is associated with reproductive complications resulting from the above or from other factors. Some of the effects appear to be intergenerational in nature. There is a white/non-white differential which is not entirely accounted for by present socioeconomic differences.

- Poverty is associated with significant greater health problems during the early years: infectious diseases, malnutrition, and by-products of living conditions such as lead poisoning.

A discussion of the impact of present health systems on these problems reaches a fourfold conclusion.

First, the federal government does not invest in children in proportion to their numbers. The basic reason for this difference is that national policy has accepted provision of a minimum level of health services as a right for the aged. Such a right does not exist for children.

Second, the free enterprise, private market nature of much of the health care delivery system is leading to specialized corps of physicians (at the expense of primary care physicians) and an emphasis on acute inpatient care in a fragmented manner. These trends particularly affect children (especially those whose families are too poor to buy protection or coordination), who need primary, preventive, ambulatory care.

Third, some of the special health needs of children—early diagnosis and treatment of chronic disease, congenital problems and handicaps, environmental dangers (accidents, lead paint poisoning), and malnutrition or hunger—have not been priorities in medical research and delivery.

Fourth, the potential impact of appropriate comprehensive health care of high quality on the child's later health status and utilization behavior has not been fully projected.

The quality of program analyses is greatly influenced by the quality of the underlying evaluative studies. Given the state of the art of health care evaluation, it is not possible to give a prescriptive list of goals and standards for children's health programs. It seems reasonably clear that all evaluations of children's health programs undertaken to date have been tentative, exploratory, and inconclusive.

However, the recent literature has been projecting models of evaluation which seem more adequate than previous models. It seems reasonable that much better evaluations could be done, at least in the area of child health programs, using only existing data techniques and methodological resources, combined with a more comprehensive approach to evaluation.

EVALUATIONS OF EARLY CHILDHOOD INTERVENTION PROGRAMS

Reviews were undertaken of the program and project evaluation literatures for the five major kinds of intervention activities on behalf of disadvantaged children, activities in: (1) early education, grades K-3; (2) preschool; (3) early day care; (4) family intervention; and (5) health care. The studies were reviewed in an attempt to determine the effectiveness of such intervention activities for

promoting the development of disadvantaged children. Repeated consideration was given to the possibility of further reviews to determine the comparative efficacy of housing and income programs for their benefit to child development. In the case of housing, a reasonable compilation of literature connecting housing factors with child development was obtained. But the literature seemed too sparse and inconclusive for reasonable analysis. No literature permitting a reasonable consideration of the influence of income programs was obtained. Consequently, the analysis followed here omits consideration of the indirect influences of housing and income programs for the benefit of disadvantaged children, although many now argue that these kinds of programs may be of great potential significance.

Early Elementary Education Projects

The primary aim of compensatory education projects has been to raise the academic achievement of elementary school children, with the ultimate aim of facilitating equal opportunity for all social classes and ethnic groups.

Reports of individual educational projects and major surveys of the effects of compensatory education were reviewed to evaluate the effectiveness of compensatory projects in early elementary school (Grades 1 - 3). There are several qualifications concerning the conclusions reached which are associated with our reliance on published information and with the limitations of the existing data. (1) Project descriptions, fundamental to our project categorizations and to our ability to related project characteristics to outcomes, were often vague and general. (2) Evaluation measures were primarily limited to the cognitive realm, to IQ and achievement tests. (3) Statistically significant gains observed were not always of certain educational significance. (4) Most evaluations have measured the effects of projects over one year only. At times this has led to judging projects as successful when, over the course of several years, they would not be so judged. Or it may not accurately indicate the possibility of projects that involve major organizational changes (which might depress achievement initially). (5) Very few projects have followed children for longer than one year or beyond the third grade.

A taxonomy of three dimensions was developed to enable an orderly consideration of the approaches to and effects of compensatory education. The three dimensions and their subcategories are: I—Classroom process (amplification of traditional classroom services, reorganization of classroom process); II—Goal orientation (academic achievement, cognitive enrichment, adjustment); III—Organizational change (parent mediation, performance contracting, busing, and vouchers).

Classroom Process. Few projects are successful which merely amplify existing or traditional services. Since most Title I projects fall into this category, the small number of successes relative to the large number of projects is disheart-

ening. Those projects which attempt to reorganize classroom process show greater success. Specifically, children participating in projects employing new instructional strategies in academic (i.e., reading, arithmetic) areas generally showed educationally significant gains; those which aimed at cognitive enrichment rather than academic goals had mixed results. Computer-assisted instruction data at the elementary level are limited, but two projects (one in reading, one in math) show promising results. Instructional television as it has been used so far seems to be as effective as traditional instruction, but no more effective. "The Electric Company" evaluations are still pending.

Goal Orientation. Except for projects with academic goal orientation, there are few data. Academically oriented projects, usually accompanied by some reorganization of classroom process, seem to be effective in increasing performance on standardized achievement tests.

Organizational Change. Busing studies have been poorly conducted to date. Overall they show no consistent effects on the achievement measures of the bused children. However, busing to achieve desegregation is motivated by complex rationales beyond improved achievement. Busing for the purposes of political socialization, assimilation, and equity cannot be illuminated by the results of IQ or achievement tests.

Educational performance contracts have not yet been fully evaluated. Two major studies (by Rand and Battelle) have reported no overall increase in the academic performance of students even though the projects reorganized classroom process and were academically oriented. The data, however, cover only one year of operation and may not provide an accurate assessment of the possible effects of performance contracting.

In parent-mediated projects, the effects of parent involvement are difficult to separate from the effects of other aspects of the projects. In general, successful projects tend to be projects with parental involvement. Parent training projects, in which parents are taught specific skills for teaching their own children, appear to be more consistently related to changing parental attitudes than projects where parents are simply involved in school activities.

Title I and Follow Through. Findings from large-scale evaluations of Title I offer little evidence of a positive overall impact on eligible and participating children. However, at the state and local level a small proportion of projects has yielded positive benefits. At least part of the uncertain results of Title I could perhaps be attributed to the lack of adequate implementation and enforcement of guidelines.

Only the first evaluation of the effects of Follow Through models has been released. Because differences between experimental and control groups are small and because of problems in the analyses, conclusions regarding the effectiveness of Follow Through must await future evaluations.

Recent suggestions to focus on compensatory summer projects have been

based on findings of higher gains on achievement tests for high SES children than for low income children during the summer months. While summer projects are a possible compensatory strategy, they are likely to be no more successful than regular school-year compensatory instruction unless they use different techniques or curricula.

Components of Successful Projects. Simply providing extra resources seems to have had no positive effect on student achievement; the important factor seems to be the manner in which the resources are used. The characteristics of compensatory education projects in the early primary grades which are common to those projects which produce significant achievement gains are: (1) clearly stated academic objectives; (2) small group or individualized instruction; (3) parent involvement; (4) teacher training in the methods of the project, together with careful planning; (5) directly relevant and intensive instruction; and possibly (6) high expectations and a positive atmosphere. Although a certain level of resources is required to maintain educational projects with these characteristics, that level of resources alone does not guarantee success.

Preschool Projects

Discussions of federally supported projects for preschool-aged children have focused primarily on whether preschool projects (mainly center-based preschool projects and especially Head Start) produce significant and lasting gains on IQ and school achievement tests. Evaluators have been forced to rely on these cognitive measures because they are the most valid and reliable tests available. Nevertheless, it must be remembered that such limited assessment of effects does injustice to the comprehensive and multifocused aims originally delineated for Head Start.

In this review preschool projects were categorized on two dimensions: goal orientation and degree of structure. Three different goal orientations—preacademic, cognitive enrichment, and socioemotional—were considered. "Degree of structure" refers to the amount of external sequencing and organization of the children's activities and to the predictability, preplanning or prescheduling of either the child's behavior, the teacher's behavior, or both.

Preschool projects were selected for review using several criteria, with the first being the most important: (1) short- and/or long-term statistically significant effects on commonly used measures of outcome, (2) replication, (3) being exemplary of a unique approach, or (4) comprehensiveness in age range of children or services. Individual project reports and other major surveys of the effects of preschool projects were used. The focus of our evaluation is on the effects of the preschool experience on the child. Few other data exist, e.g., data having to do with effects on family or community.

Effect of Preschool Projects. There is an immediate increase in IQ scores for children in most preschool projects. This increase may reflect genuine intellec-

tual progress or it may reflect increased familiarity with the situation, greater self-confidence, or an increased motivation to attempt to problem-solve in a test-taking context. IQ gains vary widely, with some projects showing much larger gains than others. The effects of most preschool projects on IQ scores do not persist beyond the second or third grade.

Children in preschools which focus on specific academic skills show an immediate improvement in performance on achievement tests. In some cases the achievement gains persist longer than the IQ increases, but typically they decline in a manner parallel to that of IQ scores. The pattern of improvement in specific content areas generally reflects the pattern of concentration within the project. The amount of improvement varies with the explicitness of objectives, the soundness of instructional methods, the time invested in attaining the objectives, and the similarity between the instruction and the performance required by the tests. Scattered results have suggested the possibility of long-term benefits of preschools, including more regular subsequent elementary school attendance and a higher subsequent likelihood of being placed in regular rather than "special education" and low-ability tracks.

Data on noncognitive effects of preschools are extremely limited and are typically based on instruments of unassessed reliability and validity. Some data do suggest an increase in desirable social behaviors.

Characteristics of Successful Preschool Projects. Smaller, well-designed experimental projects generally seem to produce larger gains than large-scale operations. The most effective projects (in terms of the measurable goals of preschools on child performance) are the most structured. Included in this meaning of structure are operational statements of objectives, consistent implementation of the strategies most useful in attaining the objectives, and perhaps as well, detailed staff planning for daily operation, adequate supervision, and commitment.

Although there has been an argument that the success of preschool projects would be increased if the age of intervention were lowered, there is currently little concrete support for this belief.

Some educators and researchers argue that preschools can be expected to produce lasting effects only if there is continuity of later educational programming, i.e., if the educational intervention is continuous. Therefore support should be provided for the development of articulated programs for children of all ages. However, the question is then raised; if preschool is not sufficient without improved primary education, is it necessary with improved primary education?

Day Care Projects

The literature on preschool intervention, reviewed just above, provides our only present basis for an estimation of the effects of developmental day care programs carried on away from the child's home for three to six year olds. Our

review concerned with day care per se was limited to an examination of day care programs on the zero to three-year-old population. We were concerned with effects on child development. It is in the 0-3 age range that day care enters into the possibility of new kinds of child stimulation. It is in this age range where public concern centers on the possibility of stimulating cognitive development on the one hand, versus possible negative effects of infection and separation from the mother on the other.

Full day programs for which there are evaluation data were categorized by intended outcome as: custodial programs (those which seek only to maintain the well-being of the child); enrichment programs (those which add a second goal, stimulation of socioemotional growth, and perhaps some exercises in cognitive development and some social services to the goal of the first category); and programs in day care settings designed to maximize a particular aspect of development (usually intensive cognitive stimulation programs, which might also include medical and social services). The programs were examined for effects on physical health, social and emotional development, and cognitive development. Data were available primarily for the last category.

The most reasonable conclusion about existing data for early day care would seem to be that the data are limited, preliminary, and inconclusive. Although there has been a significant amount of day care in this country and in other countries, the day care has been largely envisaged in terms of service to the family and there appears to have been little consideration or analysis of its effects on child development. Most of the data examined were recent and preliminary. No reports of measurable harm were found and only a few highly specialized and costly models were reported to have produced measurable benefits.

It appears that day care programs implemented within the limits of the federal and state regulations appear to be neutral in their effect on human development insofar as their effects can be evaluated by existing techniques.

Family Intervention Projects

Family intervention projects either supplement or replace child development programs in day care, preschool, elementary school or health. Goals include enhancing the physical care, cognitive and social development, and emotional sustenance of children.

Four kinds of family intervention were examined in order to assess their known benefits: parent education, parent training, family casework, and parent therapy.

Parent education projects focus on imparting knowledge (in order to improve the physical, social, and economic life of the family and hence the child), most commonly via lectures, discussion groups, printed materials, and counseling in schools, churches, hospitals. *Parent training* projects focus on skill

enchancement, especially skills believed to lead to greater cognitive development of infants and young children. Training can take place in the home only or in the home and a center. Usually, in programs with a center component, the child also attends preschool. *Family social casework* as discussed here refers only to the social service referral activities of caseworkers. *Parent therapy* is of two distinct types. Family therapy stresses the socioemotional sustenance function of parent; it has long been used by psychologists, social workers, counselors, and school guidance personnel. Behavior modification therapy for parents, a recent intervention technique, stresses the behavior of the parent in social training of children.

Effects of Family Intervention Projects. In none of the categories are effect or benefit measured without serious problems. However, more clearcut measures of benefit appear to be found in parent training, family casework, and behavior modification therapy projects. We are also somewhat more certain of the validity of the findings of these intervention activities.

- Parent education projects typically produce no useful evaluation data. In the exceptional case where useful data are provided, changes in parent behavior with direct implications for improved child development are not measured. On the basis of an assessment of testimony we find that parent education might be successful for a very limited number of families who are considered to be "disadvantaged" if the projects included day care and baby-sitting and if they were more attuned to the needs and learning styles of the particular population of mothers and fathers served. But parent education probably will never involve many fathers, and mothers who have serious survival problems (income, housing, safety) will not be responsive.

- Parent training for cognitive stimulation does produce useful, but often flawed, evaluation data. IQ or achievement score gains are usually statistically significant and of moderate magnitude. These gains decline somewhat with time but remain for at least a year or more. Trained paraprofessionals seem to be as effective as social workers or professional teachers in their parent training role. Variation in curriculum produces similar results. Important side benefits include possible IQ gains for younger siblings, less attenuation of gains, and employment opportunities for low income parents when paraprofessionals are used.

- Family casework, used for social service referral, appears to work best when supplemented by adequate income support and by an adequate level of social services in the community. Most progress is registered in "instrumental" areas of family functioning (child rearing, health care, homemaking practices).

- Parent therapy and counseling in its psychoanalytic form is barren of measured results although rich in professional testimony. It is practiced mainly

by white middle class professionals on a white middle class population. Hence it would not necessarily be useful to disadvantaged populations defined by race or low income. It is too early to decide whether behavior modification for parents is a useful strategy, although early results do look promising.

Health Care Programs

To succinctly and systematically characterize current health programs for disadvantaged children proves to be a remarkably difficult task. Virtually *none* of these programs, as far as we have been able to determine, have been evaluated or monitored in ways pertinent to this study. Several major evaluations are presently under way, but findings have not yet been published.

Given this lack of preexisting studies, the problems of describing programs and relating current efforts to critical child health needs is large. The interaction is perhaps most easily conceptualized as a matrix, having on one dimension critical child health needs or problems—such as malnutrition, infectious diseases, handicaps, or sensory deficits—and on the other particular programmatic approaches to child health—such as screening, comprehensive health, or nutrition programs. The cells thus defined represent correspondences between programs and problems. Had evaluation data been available in terms of the matrix, it would have been possible to discuss the matrix cell by cell, i.e., the specific patterns by which the federal effort interacts with the health problems of children. In its absence, descriptions of federal programs' effects in terms of child health are largely conjectural and inferential.

Programs with five emphases were analyzed: comprehensive but specifically targeted health programs (e.g., Maternal and Infant Care, Children and Youth), health screening and treatment programs (e.g., Health Start); multiservice programs with a health component (e.g., Head Start; nutrition programs; and family planning programs). In each case examples were given, and the relative effectiveness of programs both within the group, and of the group contrasted with other groups, are discussed.

Looking at existing programs against the patterns of need (i.e., needs requiring federal intervention because of inadequacies of the private sector), we find very spotty coverage of the matrix. Some programs, such as Maternal and Infant Care and family planning, are directed at both critical health needs and high-risk groups in a most appropriate way. Some programs which do not now exist in a coordinated way, such as early diagnosis and treatment of handicaps and chronic conditions, would, from evidence in other sources, have a large impact on the matrix (i.e., intervening between the critical ages one to four). On the other hand, some programs are not organized in such a way as to make evaluation in terms of the matrix possible. Children and Youth, for example, combines early infancy care with some screening with general services for older children, without a programmatic mandate to apportion inputs in these areas in relation to critical health needs. Other programs with

potentially large impacts seem to be skewed because their programmatic goals are not entirely consistent with child health needs. Thus, many of the feeding and food distribution programs do not address the issue of feeding very young children. In addition, gaps exist which no programs or nonfederal models are operating to fill. One clear need is for models which combine medical, psychological, and educational diagnoses and treatments; the failure of Head Start and Follow Through to become truly integrated and comprehensive does not bode well for other attempts, such as the newer Parent-Child Center projects. Another area which is virtually unexplored on a programmatic level is that of social illnesses in children (child abuse, neglect, accidents).

RECOMMENDATIONS FOR PROGRAM PLANNING

The recommendations offered in this report fall in two groups. For the short term, we recommend several redistributions of emphasis in program direction and management, as indicated in the specific recommendations to follow. After these recommendations have been given, we turn to recommendations for the longer term.

Given the preceding analyses and literature reviews, we recommend two thematic emphases in the near future, emphases on (1) individualizing services, and (2) working with the family rather than around it. These emphases are predominantly suggested by our attempts to analyze and define the multiple and complex nature of disadvantage in childhood. But there is testimony coming from the programs and projects as well that favors such emphases. Our more specific recommendations follow in part from the themes and in part from our reviews of the evidence and testimony. As has been suggested above, the evaluation of programs for children is an iffy business and the interpretation of evaluations unavoidably involves one in multiple acts of judgment. Nevertheless, we have made every effort to align our recommendation with existing data and to discuss data relevant to each recommendation in the main body of the report.

Recommendations for Preschool Programs (Especially Head Start)

I. Diversify Head Start away from its present primary orientation toward center-based preschool education.

- Broaden its focus to include (as many Head Start projects do) other aspects of child development in addition to cognitive and academic.

- Broaden its format to include home-based (in addition to center-based) projects, and parent training projects.
- Broaden the range of indices used to indicate its effectiveness.
- Continue (emphasize) research on the effects of center-based preschool education.

II. Implement screening programs for all children under the conditions given below. We suggest screening followed by appropriate treatment at birth, 2-3 years, and kindergarten.

- Screening should be conducted by appropriately trained personnel (not necessarily pediatricians) who work within a health system with comprehensive referral capabilities.
- Screening for and identification of needs should occur only when programs to meet the needs are available.
- Screening priorities should be based on diagnostic sophistication, the risks of late identification, and the availability of appropriate programming.

III. Provide individualized services for preschool children with special needs as early in the child's life as is beneficial. Services should in most cases include parent training.

- Currently we seem best able to provide programming for preschool children with sensory and physical needs. More programs should be implemented for such children.
- Focus on research and development of programming models where our knowledge is inadequate for current implementation (e.g., in the areas of learning disabilities, behavior disorders, or emotional disturbances).
- Implement more (and at an earlier age) bilingual preschool projects for non-English speaking children to prepare them for the regular school system.
- Adjust categorical funding at the programming level to permit integration of children with different special needs in the same preschool projects, while at the same time insuring the continued individuality of the services provided.
- Integrate children with special needs into regular school programs as much as possible, especially using special preschools to permit later regular school attendance.

Recommendations for Day Care

In the case of day care, we first consider child development issues, and conclude that:

- Day care meeting some carefully considered standard of basic adequacy will not be deterimental to children's development.
- There is virtually no way at present to know what must be added to such basic care so as to positively affect children's development generally.
- Research is needed to more definitely understand the potential and present effects of day care on children; in its absence, substantial investment in developmental day care appears inadvisable.

After a consideration of various arguments for day care, we recommend:

I. That a system of day care facilities, including centers, homes, places in private centers, homemaker services, and other facilities as outlined in the Support section be organized to deal with the needs of children from unsupervised, inadequately supervised, crisis, and stress situations as needed.

II. That some appropriate organization such as outreach services from an appropriate health care network be devised, tested, and instituted to provide screening of young children for potential health and educational problems, and that a full complement of services be made available to deal with those problems as necessary. Where research is necessary to accomplish this, it should be supported.

III. That a very limited number of densely populated areas be selected for the experimental establishment of a multipurpose day care center offering a broad spectrum of services, with both the centers and detailed analysis of their operation to be supported by the federal government. This proposal is directed toward obtaining more information on the optimal way to operate a center so as to most effectively and efficiently cope with the usage rates, types of problems, program successes, and a score of other basic facts about even a rough approximation to an average day care center.

IV. That efforts be made to produce and execute housing designs that will promote informal or otherwise shared child care arrangements. The goal is a modern urban equivalent of the unfenced middle-of-the-block backyard.

There are several more general recommendations which we state in this latter section. First, all day care, whether in homes or centers, should be of such a quality as to offer very little risk of harming the child. Second, we recognize that advances in the state of knowledge about early childhood might substantially change our conclusions, particularly in the area of child development. Analysis has shown the necessity of further knowledge about the chil-

dren; we urge that its pursuit be encouraged. Third, there might exist local situations in which a day care center is in any terms, including financial, the best solution to a group of problems. In such situations, facilities should be provided.

Recommendations for Education Services

I. Increase structure and management in traditional curriculum areas.

It is proposed that a strategy of increased structure and management in the primary school classroom be used to increase the attainment of basic skills of reading and arithmetic. Increased classroom structure and management includes: (1) a strong instructional emphasis with clearly stated and measurable goals, which are carefully sequenced; (2) ongoing assessment capability in the classroom; (3) individualized help after assessment; and (4) extensive planning by and careful supervision of the instructional staff.

The federal government could move to encourage increased structure and management by providing incentives primarily in the form of (compensatory education) resources tied to conditions designed to encourage adoption of the recommended strategy, by providing technical assistance and information, and by creating demonstration projects.

II. Diversify education.

To diversify education essentially means to broaden the range of activities emphasized in the classroom which are considered a legitimate part of the child's education and for which the child is rewarded and receives prestige. Those skills that are now considered basic should be taught in the most effective manner possible. But the remainder of the day should be spent in activities which allow each child to use and develop other skills. It appears that there are other human skills that—in terms of vocational relevance, in terms of the structure of human abilities, in terms of educability—fully deserve to be a part of the basic early curriculum.

This recommendation can now only be given in a general way, as a statement of the need for the development of a diversified emphasis. The types of activities and the specific curricula which would be included in a diversified education program would have to be established by a development program. The established distinction between verbal and spatial abilities could well be used as a starting point, given that the distinction has been well documented and that a reasoned argument now exists that schools should acknowledge it. We are here recommending planning followed by program research and development in order to design a more diversified education for children.

Recommendations Relating to Family Services

This recommendation proposes a general goal of services which should apply to planning and implementation of all public programs directed at children. The main thrust of the recommendation is toward working with the family, rather than "around" it. We hold that public policy in this area should make families the focus of intervention efforts, should use parents as primary agents of change, and should involve parents in policy and administrative decision-making. A series of specific procedures are presented in the body of the report to support this recommendation.

Health Care Recommendations

Children's health problems are not neatly separable from problems in the organization of services designed to solve them. Consequently, health recommendations cannot be made through a comparatively simple comparison of existing problems with available programs. Generally speaking, in the case of health the right programs exist. The problems arise from utilization, availability, and accessibility. Health care recommendations must reflect a consideration of the health caretaking system.

From a general introductory discussion of the relationship of health to child development and the problems associated with policy decisions, we move to a set of recommendations for child health programs related to the general child development strategies which are being developed in this report.

Recommendations:

I. Nutritional programs should be redesigned, expanded, and given greater priority as a preventive health strategy for children.

II. Maternal and infant care projects and family planning programs should be expanded to cover more of the high-risk populations; these programs should remain (as they are at present) separate, categorical programs for the immediate future.

III. Other direct health services efforts for children should be incorporated into one of two more comprehensive settings:

- Comprehensive, family-oriented health delivery systems such as Family Health Centers.
- Multi-service programs for children, such as Head Start, schools, or Parent-Child Centers.

IV. Broad emphases in a child health strategy should be: (1) diversification of pediatric manpower (along with general expansion of allied and community health personnel); (2) improvement in financial support for child health services; (3) and improvement in general environmental conditions for children.

COMMENTS ON FUTURE ANALYSIS

The data base for a study such as this consists of three kinds of research activity: (1) analysis, (2) problem and program studies, and (3) basic research. How could one facilitate planning in the future?

We suggest that a permanent intramural analysis group be set up within HEW to provide for a continuing synthesis and analysis of information about programs for children. This group would not be concerned with day-to-day planning activities, but it would have periodic responsibilities to provide a comprehensive analysis of programs for children. We suggest that one or two extramural groups be established to assist in the development of analyses, and to provide possible other perspectives about program guidance.

We do not make recommendations about the futherance of problem and program studies and of basic studies, because there are present trends toward a greater quality and quantity of relevant research activity in these areas. This seems manifest in the development of agency planning functions, and in the move toward interagency coordination created by the Interagency Panel for Early Childhood Research and Development.

There seems to be a division, roughly, between the kinds of problems faced by traditional child welfare legislation and programs, and many of the newer group of problems brought in by the recent poverty initiatives. The former are problems of children in trouble because of personal crisis or risks in the immediate environment; the latter are problems of children who have statistically poor chances of social status later. These are termed "child welfare issues" vs. "social issues."

It is probable that the "social issues" are not uniquely issues of childhood, nor uniquely to be addressed by analysis or programs confined to childhood. Such problems relate to the following questions:

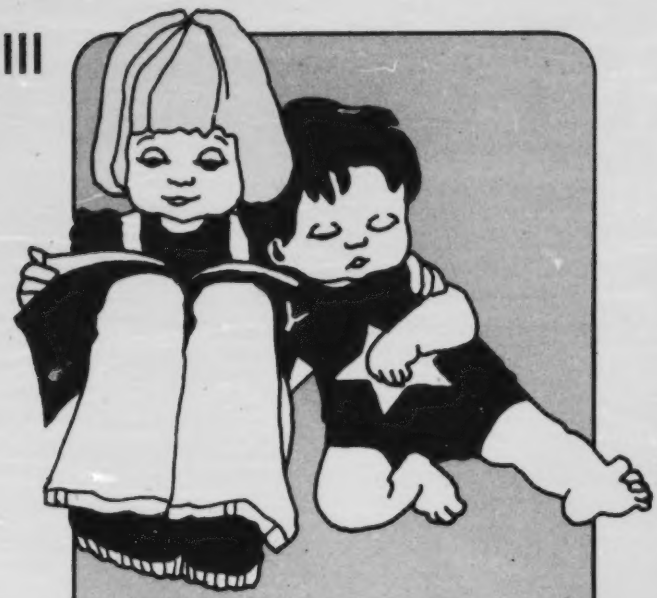
- Do we have more education than we need? Do we have the kind of education we need?
- Are the existing child care professions still fully viable? Can we solve problems by multiplications of them? Are the professional structures the reasonable sources either of problem definition or of advocacy?
- Can social institutions replace the family for the child? Is it possible for us to strengthen the family in its relation to the child?

- Can we provide more openings for productive labor? Can we provide more distributed dignity of labor?
- Can we create a political socialization?
- Can we find some way to remain competitive as a society without crushing competitiveness within? Where and how can we assert "quality of life" values for families and their children?

The "child welfare issues" now dealt with by programs for children can probably only be solved by efforts to obtain the long-sought-for services that are individualized, relevant, comprehensive, and coordinated. This will probably not be established by new programs for comprehensiveness established on a historic bed of old programs. Most likely, it will require changes in the management of existing programs.

Probably, in the long run, effective services could be obtained by extensions of existing health and school service bases. The fundamental innovation needed is an effective provision for local management and accountability.

PART III



Critiques, Summary, and Remarks on the Report and Conference Proceedings: Implementation of an Interactive Model

The Child Care and Public Policy Conference held in March of 1974 focused on Dr. Sheldon White's *Federal Programs for Young Children: Review and Recommendations*, which is summarized in the previous section. What follows here are four critiques by distinguished scholars in the field, a summary of these presentations, and a statement using these comments to link the issues of child care and public policy.

It was hoped that by convening this group of peers and policymakers, discussion of White's paper would highlight the complexity and diversity of the problems in this area. Most importantly we needed to make it clear to all concerned that there is no single answer and also that there is no single question.

Each of the following presentations reflects the particular interests and expertise of an individual scientist. While they are not necessarily divergent opinions, they do represent different concerns.

Commentary on *Federal Programs for Young Children: Review and Recommendations*

Carl Bereiter
Ontario Institute for Studies in Education

Were it not for the widespread uncertainty and gloom currently surrounding early childhood education, this would be a discouraging report. Like other recent reports it finds that nothing seems to make much difference, but it does not stop with this facile and disheartening observation. It is impressive that Sheldon White and his colleagues sifted out so much relevant policymaking material from the available data. Specifically, the findings I refer to are that structured teaching programs have tended to be more effective than unstructured ones, that parent training has been more successful than parent education, and that referral has had more tangible benefit than therapy in family social work. I will comment on several of these conclusions with the intent of modifying or amplifying them in the light of my own knowledge and judgment.

First, however, I would like to discuss a vital issue—the measurement of educational outcomes—which is treated merely as a background topic in the report. The entire effort of policymakers to base their decisions on data will be determined by the ability to measure relevant outcomes. The measurement picture is somewhat bleaker than the report indicates, and on at least one point this bleak picture has definite policy implications. Briefly, the measurement picture may be summarized as follows.

1. As the report indicates, present measurement technology is adequate for the assessment of intellectual aptitudes but with two major qualifications. First, cross-cultural comparisons of aptitudes are very risky. This does not mean, however, that available measures are invalid for certain cultural or ethnic groups. Most available evidence suggests that existing aptitude measures are about equally valid for American subpopulations and can therefore appropriately be used for comparing differently treated groups of children within given subpopulations. What is not appropriate is to judge the success of an educational intervention program for poor black children, for instance, by comparing aptitude test scores with middle-class white norms. Failure to make this distinction between within-group and across-group comparisons has led some policymakers to reject aptitude tests altogether when they might still have valuable uses for within-group assessments of educational effects. The second qualification, however, is that we have no basis for

judging the value, if any, of experimentally induced changes in aptitude test scores. A recent study of large, representative first-grade populations indicates that in the last 10 years scores on a reading readiness test have gone up enormously, with no attendant increase in the reading-achievement scores that the readiness test was supposed to predict. This finding gives substantial weight to the fear that changes in aptitude scores may have no material consequence and therefore should not be taken seriously as indicators of the effectiveness of early childhood educational programs.

2. Our ability to measure achievement in basic scholastic skills is probably much lower than the report indicates. Elementary school achievement tests have embarrassingly high correlations with measures of scholastic aptitude. Of particular significance is the close relationship between tests of reading comprehension and tests of verbal intelligence. No one, to my knowledge, has succeeded in demonstrating that these two kinds of tests measure anything different, which leaves us in the position of not being able to measure with any confidence what is without doubt the single most important skill objective of elementary education. It is most distressing that specialists in educational evaluation are pursuing high-flown models of the evaluation process while doing little or nothing to raise the basic instrumentation of measurement above its current unspeakably inadequate level.

3. The report correctly indicates that our ability to measure outcomes in the area of personality and social development is virtually nil, and this is not from a lack of efforts to devise tests. The report hints at the prospect that personality and social development outcomes may not be susceptible to testing as we now conceive of it, that is as something that can be accomplished at a sitting and at modest cost. There is no reason why it should be. Finding out whether a person *can* do something is a much different matter from finding out his response tendencies in a broad area of behavior. While finding out about the former may cost a dollar, finding out about the latter may cost a hundred or a thousand times as much. Accordingly, if policymakers are serious about wanting to base their decisions on empirical results in the personality and social domain, they may have to be prepared to make an enormously larger investment in evaluation than anyone has seen fit to make thus far.

4. The new approach to evaluation signified by the "indicators of quality" and enthusiastically reviewed by the report is not an advance but rather a dismal retreat from the problem. It amounts to taking what had been considered independent variables (teacher behaviors) and turning them into a dependent variable ("good teaching," as judged by experts). With such an index any program can be converted from a failure to a success simply by conforming to the prevailing prejudices. Such a criterion would generally stifle efforts to improve the state of the art.

The purpose of these remarks on evaluation has not been to engender despair but to suggest that if policy decisions are to be based more on outcome data, as I believe they should be, efforts to develop basic measurement technology will have to be much more serious and at a much higher level of support than they have been in the past.

I will now turn to several of the report's major recommendations that are related to preschool and elementary education.

"Diversify Head Start away from its present primary orientation toward center-based preschool education." Some specific and reasonable suggestions are made for extension of Head Start activities beyond regular preschool education, but the report remains vague about what should become of the center-based preschool programs that now constitute the core of Head Start activities. Should they be expanded, held to their current level, cut back to make way for the more specialized programs suggested, or ultimately phased out? The same inconclusiveness is discernable in several chapters in which evidence bearing on the need for early educational intervention is presented. Whereas there is little to quarrel with in the analysis of evidence piece-by-piece, it does lead inexorably to a conclusion that is left unstated. The conclusion may be formulated as follows. Ten years ago there was no decisive evidence but an abundance of suggestive evidence supporting the hypothesis that early educational experience is crucial for cognitive development. After a decade of extensive basic and applied research, we still do not have any decisive evidence, but the trend of suggestive evidence has been rather consistently *against* the hypothesis. In short, we now have much less reason to believe in the importance of preschool education than we did back in the days when our knowledge consisted of that summarized by Hunt's *Intelligence and Experience* and Bloom's *Stability and Change in Human Characteristics*. This conclusion, coupled with what appears to be encouraging results from Follow Through, leads me to favor the eventual withdrawal of resources from preschool education in favor of the improvement of instruction in elementary school.

"Include home-based . . . and parent training projects." The problem with this recommendation is the assumption, almost universally shared, that home intervention is merely an alternative and hopefully more effective "delivery system" for services otherwise comparable to those of a center-based preschool program. The two are radically different. Compensatory preschool programs simply add on certain experiences to those already provided by the home, leaving the home essentially untouched. Home interventions aim at changing, to a greater or lesser extent, the culture of the home, especially the character of parent-child interactions. The courts have long recognized a division of authority between the state and the parent in socialization of the child, with the general recognition that parental authority is primary, to be abridged only to a minimum extent necessary to meet society's needs. Home intervention constitutes a potentially major shift in the balance of authority, in favor of the state,

at a time when there appears to be a shift of values in the opposite direction—in the direction of pluralism and against the homogenizing influence of state-controlled education. The profound human rights implications of home intervention ought to be seriously considered, and decisions should not be merely based on cost-effectiveness.

"Increase structure and management in traditional curriculum areas." This is an important positive recommendation. It may be premature, given the current state of evidence, but it is clear that evidence does converge in this direction. However, it is not known that structure alone acts independently to improve the effectiveness of school programs. Although programs effective in raising school achievement tend to be structured, not all such programs are effective, and what makes the difference in effectiveness of structured programs is little understood. (It is quite possible that an examination of Title I findings would show more ineffective than effective programs.) The report has been overly hasty in rejecting curriculum and curriculum materials as an additional and perhaps interacting variable. The evidence cited for the irrelevance of curriculum variations consists largely of statements by people who have not, to my knowledge, carefully examined the research. In the area of reading, for instance, the evidence in favor of intensive phonics instruction is at least as strong as the evidence in favor of structure. Intensive phonics instruction, of course, virtually necessitates a structured teaching method, but the reverse is not true. There may be a constellation of features that make an effective teaching program; structure is one aspect of such a program that is insufficient without the others.

"Diversify education." This recommendation is so broad that it is little more than endorsement of a catchword. There are several directions that such diversification might take, but of these the one suggested in the report seems the least promising. I refer to selecting some neglected mental ability, such as a spatial ability, and attempting to foster it. This sort of thing has been done in connection with various perceptual abilities, with skills nominally measured by the Illinois Test of Psycholinguistic Abilities, and earlier in connection with Thurstone's Primary Mental Abilities. Generally it has been possible to design training to raise relevant test scores, but so what? There is no evidence to suggest this is a meaningful or useful kind of education. A more significant kind of diversification could arise from designing new instructional programs for teaching core subjects that would draw on different abilities from those that are now predictive of success—for instance, to devise a way of teaching reading to children who are high on spatial but low on verbal abilities. This is the kind of programmatic effort that Jensen has proposed and that is implied by research on aptitude-treatment interactions (going out and producing interactions, that is, instead of just looking for them). At present, however, we have no idea if this is even possible. In spite of decades of talk about adapting instruction to individual differences, almost nothing has been done about it.

The report is quite clear in saying that much research has to be done before diversification can be realistically implemented. What the report does not make clear is that the day for intelligent diversification of curriculum and teaching methods is so far off that in the meantime we need to continue devoting a great deal of effort to what Dr. White refers to disparagingly as "solving the average child's average problems."

I will not comment on the report's recommendations concerning day care, health, and family services. They seem very judicious and practical, but they deal with matters outside my zone of competence. There is a certain continuity to the wisdom of these recommendations which deserves to be heeded over and above the particular proposals. The essential wisdom emerges most clearly in the recommendation that "screening for and identification of needs should occur only when programs to meet the needs are available." Such a statement is self-evident and would not need to be made were there not such a history, in programs for children, of directing money toward the most loudly proclaimed needs instead of toward the means that show greatest promise of doing some good.

William Kessen
Yale University

An omnibus story that is a favorite of mine concerns a very popular college course in Old Testament literature that was well attended because the instructor invariably asked the same question for his final examination: Name the seven kings of Israel. When he learned at last that he was the butt of campus jokes, the instructor without forewarning changed the examination to a question about the philosophy of the prophet Samuel. All but one student left the hall at once; the staybehind wrote bravely on his paper, "I do not know much about the prophet Samuel, but, for what it's worth, here are the seven kings of Israel." Under present circumstances, those of us attending a meeting on public policy and child development may behave somewhat like that student—giving our answers to Dr. White's report without regarding the specific questions that it raises.

There is further poignancy in this story with regards to our task. This prodigious report on federal programs for children certainly can be seen as representing the seven and the seventy-seven kings and queens (and perhaps the knaves) of child development, and yet it also very much needs a Samuel. Unfortunately, the report does not have an explicit message. *Federal Programs* is not the kind of statement we considered in 1973 as fulfilling the intention of the forum on policy; it is no Jencks or Jensen. The manuscript is too sane, too bland, and polyarchal to arouse strong feelings. Although it will lead us to state our prejudices and our points of view, it does not provide us with the kind of clear target we might have wished to support or to contest.

There is much, however, to admire in this remarkable report, this twenty-camel train. The hard-headed attitude towards costs and benefits, language which I do not usually find congenial, and the reduction of an enormous data base from two thousand pages to a two thousand word recommendation for policy is a substantial achievement, whatever its implications for policy.

After consideration of the vast literature, White and his colleagues come to two explicit conclusions—that the goals of federally supported programs for children should be shifted toward greater individualization and responsiveness to children's needs and that the programs should work with, rather than around, the family. Not quite as sure-handed is the recommendation that federal programs should be directed in the first instance toward the culturally excluded and the disadvantaged. There are other and perhaps equally important implicit conclusions which I will address shortly.

Whatever our consensus about the general conclusions, there is much in the report to be happy about. The emphasis on early health screening, for example, is fundamental and exigent. The proposal to integrate children with special needs into regular classroom settings as soon and as often as possible may not be viewed sympathetically by many groups, but it certainly deserves our discussion and, I believe, our support. Not surprisingly, however, there are parts of

the document which can be argued—specifically, the puzzling recommendations about day care. Rather than discuss specifics, I will raise three general issues: the rule of the culture, the central issue of values, and the paradoxes of adaptation.

The Rule of the Culture. A striking characteristic of this report is its strange disconnection from the major forces at work in our culture. To exaggerate, it is almost as though the authors had walked into a great barn of a library called "child development" in which there were neither exits nor windows. Two illustrations of such disconnections should serve to make my point. The primary changes of the last 50 years that affect child care, and by implication, federal programs in child care are changes in the status of American women. No discussion of the issues can proceed sensibly nor can there be an understanding of the problems of children without initial and continuing consideration of that historical mutation. The fact that more and more women will work, that child care in the home has begun to be less gender-differentiated, and that there are new expectations of women and of men all bear directly and immediately on child care and federal programs. The future of American women, some of whom will be mothers, is not an esoteric or academic issue; it is far more practical and immediate than whether we teach Piaget or Bereiter.

The second instance of disconnection is perhaps less obvious; it is the failure of the report to recognize changes in the function of children. Here I draw heavily on the recent work of Professor Keniston and his colleagues at the Carnegie Council on Children. They have plotted, with some success, the fundamental change in the role of children over the past century from being economic assets to economic liabilities and the parallel culture-defining change from having children to fulfill a moral obligation or economic necessity to having children, to use an ugly phrase, as a consumer pleasure. Whatever their theoretical implications these are crude practical issues relating to population growth, patterns of age distribution in our culture, and the goal expectations of parents for their children.

I refer to these omissions and other less notable ones as "the rule of the culture" because, however painful it may be, it is true that we can consider child care only within a cultural context; as late twentieth-century Americans our definitions and viewpoints are embedded in a cultural context. Some of us, certainly those of us who come out of an experimental tradition, would like to be able to isolate the problems of child care and development and to deal with them as though they were freed of their entailment in the rest of the culture. However, we cannot and therefore, the report should have been addressed even more so to the cultural contexts of child care. Incidentally, three of us at this conference recently visited China and are even more convinced of the notion of the rule of culture than we were heretofore.

The Central Issue of Values. The second group of general issues make up the central issue of values. There is a tension in the report between the explicit

goals stated in the earlier parts of volume one and the implicit goals which actually define it. To put it straight out, I believe that the most important questions about child care are ethical and ideological, not technological. Yet, there is a wariness in the report about addressing the ethical and the ideological, and surely the reluctance has a sound defense—to maintain scholarly distance, to be objective, to be reasonable. But the assumption that psychological facts are free of value and valuation is nonsense; in the case of child care, it is pernicious nonsense. And, ironically, what happens when we try to be standoffish and impartial is that the ideology sneaks in and you find, to take the report in hand as an example, that the primary ideological and valuational definition of the document is never explicitly stated.

The unseen argument has, in my opinion, two premises. The first is that we can respond to the individual child's needs because (a) we know what they are and, knowing them, (b) it is appropriate to respond to them in spite of competing considerations. To make the contrast obvious, there exist, in addition to the needs of the single and particular child, the needs of the group, the needs of society broadly bounded. It is not my intention to support one value over the other; rather, it is essential to note the unexamined ethical premise of the report that leads, psychology and technology aside, to an emphasis on the further individualizing of child care and educational programming.

The second ideological premise, which is not always made explicit, is the goal of catching up with middle-class groups—catching up the disadvantaged or the excluded to match middle-class standards. It is strange that both of these ideological principles are part of our blood, but they become dangerous only if they are permitted to remain unexamined. Yet, we may conclude, when we have completed our deliberations, that these are basic ideological principles that we wish to maintain explicitly. I propose only that we be certain about our convictions, particularly about the catching up premise; there are logical as well as ethical problems with this racecourse notion.

It is interesting that concerning the question of values, the report is strongest in its recommendations about health services. In spite of the absence of any formal evaluation of existing health care programs, the report makes excellent sense, is quite persuasive, and well directed. The apparent consensus of opinion is probably due to general agreement as to the value of healthy children; health may be one of the few remaining values that most members of the culture share. Whereas attempts to achieve agreement among Americans to such assertions as "God is good" fall short of unanimous assent, everyone agrees that you should brush your teeth daily. This statement not only reflects American attitudes, but also the problems we face. We can move ahead on a federal program of child care only when the program is based on some shared principle of the culture. It is a truth politicians learn early and one we cannot ignore. But more to the point, if we do not have any strong sense of shared values or a productive and signaling idea about the values of children or the values of education, then, in such a policy of entropy, we move towards indi-

vidualization. More bluntly, whatever its other sources, individualization represents the absence of positive ideology and the weakness of shared values in the United States. This report represents a far more widespread failure to recognize the ethical and ideological dimensions of our work, a failure which has led in turn to the curious texture of our field.

Witness, as example, the engineering model, implied by White and his colleagues, that is slightly shifted away from an emphasis on the developmental psychologist as expert. Imagine that the king (I am obviously obsessed with royal images) says to the Royal Railroader, "You folks are supposed to be the experts on railroads; please make some recommendations on what we should do about national railroad policies."

The rail experts make an intensive study of railroads, come back to the king, and say, "Well, there's an enormous variety of track—we have some narrow gauge and some wide gauge. We also have a wide divergence in equipment and patterns; some trains run only between Peoria and Chicago, others from Princeton to New Brunswick. We are not always able to tell if the trains work or not because we haven't been able to get some of the engines started. Then, of course, some of them that have started haven't been working as well as others although our rail-evaluation procedures cannot determine the reasons for the variation. But we do have two strong recommendations, your majesty. First, we think we ought to maintain the variety in our railroad system—another example of democratic pluralism—perhaps even increase it. Second, we surely ought to keep the pullman car!"

In the meeting of experts and policymakers nobody asks, "Where do you want the trains to go?" American child care and education may not be quite as diverse as this example suggests and some of you may not agree that the defense of the family sometimes seems as quaint as the defense of the pullman car, but the general implications of the fable are important.

In contrast, Alexander II once faced his engineers and, asked about the design of a railroad between St. Petersburg and Moscow, seized a straightedge and a pen and drew a line with neither curve nor contour. Neither advocating such autocratic solutions nor the similar solutions of democratic centralism, I do feel that we must face the problems of living and working in a pluralistic society that has no positive ideology. Even more appropriately, we should be wary of the report's implied analogy between applied developmental psychology and applied physical science. Such an analogy is crippling because the psychologist cannot function as a technocrat in the absence of ethical or ideological imperatives. Moreover, the psychologist does not function well as a technocrat because of a not very well hidden desire to be policymaker as well as advisor on the implementation of policy.

The Paradoxes of Adaptation. And so, in almost orderly fashion, we reach the final set of issues, the paradoxes of adaptation. Although there has been ritual bowing to Darwinian or adaptational conceptions of child development for 69

many years, few psychologists have taken the model seriously in its detail and in its precision. If there were time enough, I would try to make a strong case for the re-awakening of an adaptational model for child development. Short of such an exercise, I will use the concept to suggest a problem we face if we see the child adapting and, while in the process of adaptation, constructing his own environment, and then adapting to that environment in a continuous fashion throughout his life. The easy contrast between sensitive periods and hereditary determination seems absurd; we much face the complexities of a child in constant adaptation to an environment which he both meets and transforms. However, if continuous adaptation is a proper view of the child and if human beings are as adaptable as I think they are, then we have a remarkable paradox.

The first part of the paradox is based on experience with the adaptational capacity of children. In our own children and in the children we see in schools or other institutions, remarkable transformations are apparent. Children learn remarkable things; they become very subtle social theorists and so well-attuned to their environment that they make discriminations child psychologists can hardly begin to plot. Children are not only adaptive but also superbly competent in their adaptation.

The second part of the paradox, which is allied to the first, is the vast cross-cultural differences in children and child care. Bronfenbrenner, Stevenson, and I can present detailed arguments, for example, that Chinese children are profoundly different from American children, and unless one is prepared to make radical cryptogenetic hypotheses, one must maintain that the cultural differences represent different adaptation. Against these two parts of the paradox must be juxtaposed a third—the apparent ineffectiveness of all our early educational programs. If we have this organism adaptable beyond all others, this child who can sense and conform and construct remarkably complicated arrangements, why do we have this report, thousands of pages in length, which informs us that no matter what is done, there will be little difference? The paradoxes of adaptation or docility ought to be the center of our attention. The only resolution of the puzzle that makes much sense is derivable from the first two parts of the paradox—we have not arranged our educational programs so as to make sufficient and appropriate demands for adaptation on the part of the child. At present, he is able to solve almost any of our educational innovations by role-switching, by "code-switching" into the setting where we put him and then switching back to his other definitions—the definitions of the family, the street, the peer gang, personal psychopathy—as soon as he leaves the educational setting. Individualization, even to achieve its own limited goals, must be an individualization (an indivisibility making) across all the living settings of the child. But, inevitably, the circle turns and closes; we live in a culture and at a time which carries a strongly held and shared belief in sprawling individualism and the virtues of fluent role-switching.

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Educational Testing Service

The issues this report raises are many. I would like to focus my remarks on the particular items that I consider relative to policymaking and a knowledge base. I am most impressed with the scholarship that went into this report and its interesting historical perspective. This helps us to gain an historical orientation which will alleviate, perhaps, some of our impatience with the rate of social progress and education. The questions I would like to address in my comments are as follows:

1. Who are the policymakers we are trying to "educate" or, more accurately, wishing to work with?
2. What kind of information do policymakers have to have to make a decision, and as a corollary, what form does this information have to have?
3. What do we do with policy issues when there is little information?

First, I wish to emphasize that the belief system of policymakers is a critical factor in our discussion. I believe that policymakers have a set of constructs by which they define social and physical reality. Such constructions of course are not unique to policymakers but are characteristic of each of us. The point at issue is that the belief system of policymakers is something that we know little about. I believe that we can make certain assumptions about policymakers that would be relevant, but the substance of these comments is yet to be uncovered. For example, I am certain that every adult—policymaker or not, parent or not—has some ideas, attitudes, and beliefs about child care and the necessary and sufficient conditions for adequate child care. For those who are parents, the feeling becomes more relevant an issue because each feels qualified, by virtue of his or her experience, to make general statements about child needs and child care requirements. Thus the implicit knowledge or belief systems of every policymaker will influence the ways in which he treats new information. My argument, therefore, is that this is a critical variable in our working with policymakers and in presenting a report of this kind.

Related to this is the fact that child care is deeply embedded in a context of social values and social aspirations. Attitudes and belief systems of policymakers or citizens have to be viewed in the context of what the society wants for its children and what responsibility the children have to the society. Finally, there is concern with the nature of the belief system of policymakers vis-à-vis the role of research in the social sciences. Do policymakers understand the appropriateness of research regarding such issues as program effectiveness and child care policy questions? I am not sure that there is public acceptance of such research. When we use such terms as experiment, manipulations, and treatment conditions, are they accepted by the educated public? Does the

public believe the methods of science to be appropriate to the areas that concern us? Does research merely represent to them more statistics gathering and description of trends?

The above comments are relevant in that they underscore the need for us, as investigators, to better present our findings if we are to reach the public. I am not talking about the complexity of research results or statistical sophistication. Rather, my point is exemplified by the following. Head Start is based on the premise that children from certain segments of our society come to school unprepared for the demands of school due to certain environmental characteristics and experiences. It is conceivable, however, that one could justify Head Start not from this point of view but from the point of view that these children are basically slow and genetically limited. Based on this assumption, it takes more time to educate these children; therefore, by starting early, they can be provided with the experience necessary to function in school. A policymaker, then, may support the Head Start program for reasons other than the explicit rationale for such a program. Whereas the rationale for such action may vary among policymakers, they may agree on the action. If this is true, then the evidence they seek would be different and the information they would ask of researchers would also differ. If we do not know the perspective of the policymaker regarding this particular issue, we may give him information that he cannot or will not utilize.

I would now like to raise some issues regarding the context in which this report can be considered. It will be recalled that the report focuses on federal policies and points out very poignantly the wide array of federally funded programs for disadvantaged children. Yet, the society in which we live is so diverse that we must raise the question of the relationship between federal programs and local customs and practices. I am fully aware that for some programs like desegregation, the local option approach may have not resulted in the desegregation gains evident today in the South. Ironically, there is at present probably greater desegregation in southern cities than in many northern cities. This may be cited as evidence of centralization and flouting of local policy. One can easily envision preschool or day care programs that will also vary by community. The needs and approaches to day care in rural Kentucky may be far different than in Boston or Princeton. In essence, my point is that the issues described and the policies recommended must be presented against a backdrop of decisions regarding local options.

I find it very difficult to think of social policy concerns as separate from the larger social context. The report does deal with this with some consistency, but it does not come to grips with solutions. That in fact may well be the role of this meeting. We are representing a variety of interests and we do have the chance to begin a dialogue; perhaps we can move to some practical solutions. It seems to me that the solution to some of the social problems described in this report are the responsibilities of agents other than those listed. The responsibility rests with business and education as well as the government. Oppor-

tunities for work, advancement, and satisfactory incomes are determined by a broad array of individuals. On the other hand, the schools have a decided responsibility in facilitating the entrance of children into the world of work as well as into the world of social living. Thus, my contention is that solution to the problems posed in this report must come from broader segments of the society working in smaller units than on the federal level. The problems, of course, are complex, but we must look to the relationship between the federal government and other units of government.

The issues I would like to present are the following: 1) federal standards tend to be higher than local standards (nursing homes are an example); 2) there is a danger of vested interests in local groups becoming dominant and determining policy; and 3) the need for objectivity in evaluation. I think there is need to define a new relationship between federal and local interaction. I think many of us have tended to favor federal policymaking because on the whole it has been not only broader but probably more liberal. On the other hand, I can see serious problems emerging with federal attempts to establish uniformity in some social policy areas.

But before we proceed with these points, we have to know who the policymakers are. They are so varied as to include members of the board of education as well as classroom teachers. And yet every parent voting in the community is also a policymaker.

In a democratic society, everyone is or can be a participant in policymaking decisions, and in a complex industrial society where policies are made at every level, layer upon layer of policymakers exist. The problem of identifying policymakers is indeed complex and perhaps insoluble. A teacher reads an article on successful learning in the classroom. The teacher decides that the specific approach makes sense, but achieving the same results is a problem. I feel there is a tremendous wasteland between the knowledge resulting from research and the implementation of that knowledge. Teachers have a better understanding of what they need to achieve their goals.

The implications of this report lead me to the conclusion that there are two broad categories of research, each requiring different strategies and complements of individuals. The first is social policy research, and the second is basic research within particular disciplines. The social policy research that I feel is needed must be interdisciplinary in nature. All of the policy issues and the problems of application of research have to be in a sphere that takes into account the multidisciplinary nature of the issues. For example, to understand social change and the role of change agents involves not only social psychologists but also sociologists, economists, and others as well. I find it difficult to think of any social policy question that can be solved conceptually or empirically through the efforts of one discipline. I am fully aware that the concept of interdisciplinary research has been around for a long time, that it has posed many problems, and disillusioned many. Yet, it is difficult for me to see now how solutions can be applied to the myriad of social problems by our tradi-

tional research methods. It is perhaps belaboring the obvious to point out, on the basis of this report, that the approach to many of the social problems and the solutions recommended require interdisciplinary efforts. For example, the trouble with the evaluation studies in much of the preschool material is the lack of weighting of the influence of the home with the school. Do certain homes undo what the school is trying to accomplish? How were families in Head Start programs studied? Were certain variables selected for study that would be the most promising with a particular disciplinary orientation?

I must reiterate my belief that variables selected for study by anyone are by definition biased and outgrowths of one's Weltanschauung. The theoretical bias of the investigator influences the conceptualization. Thus, the Olympian view one might have about all the gross areas that are relevant for examination—the family, the parent-child relationship, and so forth—is not sufficient to lead to the selection of the most relevant variables. One could argue then that by varying the individuals in a research team, the diversity that is sought is thereby created. After all, psychologists have been known to disagree with each other. I think this is more than an issue of individual difference. The issue is that the type of training received and values held leads to different conceptualizations of the social realm. In effect, this supports a strong plea for interdisciplinary teams to study social problems.

Let me suggest one area requiring interdisciplinary investigation—the physical health of family members. I am not conned by the Geritol ad into believing that if you have your health you have everything, but I do believe that basic medical health is crucial to many of the children under consideration. Health of family members then is one way of defining the family environment, and we need to know the impact of this condition on subsequent activities of children. The view and treatment of health problems as well as the myths and beliefs about health create an interesting network of interactions which call for psychological study. We can further extend this to the medical educator—the nurse or the physician who gets involved by providing treatment. A team approach is logical in dealing with issues related to health. I am sure we can set up prototypes of models we are recommending for implementation into this research area.

Research within a discipline is more straightforward and has a tradition that perhaps needs examination. Some of you may argue that the advocacy of research in the traditional disciplinary fashion with the traditional methodologies is archaic and in need of drastic overhaul. Maybe we are in an era of change in social research paradigms. My personal preference is for more diversity, with mutual interests in laboratory as well as real-life settings. The press should be for selection of appropriate and relevant problems which perhaps in themselves will dictate the kinds of research paradigms that are needed. For example, it is clear in my thinking that we know very little about the impact of group experience on the development of a child's concepts of self and others. Further, I believe that this can be studied in a relatively controlled manner in a

setting that overlaps with real-life educational situations, in this case the nursery school. We plan to set up our nursery school research program with this as an important ingredient. I think the research paradigm we are working with is quite traditional, but the novelty perhaps is in the problem selected for study. Support for this type of research is necessary. Research granting agencies, however, must be fully aware of the complexities in carrying out this type of activity, and we do need policies which further rather than hinder research. I am certain that many of us in this room can point to illustrations of federal policies in research granting that hinder the development of research in the direction and of the level desired. The nexus of the research activity within disciplines and the social policy issues is in the development of policies which are, literally, in the service of a research tradition rather than in opposition to it.

Along these lines, I would like to add to our agenda consideration of social policies regarding research. From my experience, for example, NIMH tended to be a conservative research granting agency, with low-risk studies in terms of outcomes. I understand their cautiousness due to the fear of being scapegoated or criticized for indiscretion or high risk. On the other hand, where does the support for high-risk experimentation come from? Criteria may be altered, especially when much of our early childhood research does have a low risk in the sense that conventional assessments tended to predominate and were coupled with archaic objectives. We could turn that around by maintaining programs that seek more innovative ways of evaluation. Thus the risk is not in the program effects but in the kind of data that is obtained. I am certain that many data—such as teachers' records and children's productions—were discounted quite unwittingly, because they do not fit into the conventional mold. It seems unfortunate that we stick to such old-fashioned and restricted methods of evaluations.

After reading this comprehensive report and its excellent review of the research literature, I am left with the feeling that much of what is here has to be distilled for the policymaker who is not a social scientist. The problem is how to implement many of the recommendations. It seems to me that we need at least three types of task forces. One is the action type that would work closely with individuals who can translate not only the research but show some kind of practical way of working with it. For example, if research data shows that there are some deleterious effects of day care for children from three to six, then these individuals must fully understand the research data if they intend to influence a community to accept day care. In such a case, it is not only the policymakers who need convincing but also the potential consumers who have to be convinced that day care will not be deleterious for their community. This is a good example of the difference in local control. In a study we did in an area of New York state that was considered part of Appalachia, groups of parents felt that day care was not functional as an educational medium but merely useful as a place for children of single parents who needed to work and have a place for their children. It was viewed as a necessary institution for a particular

class. Why? Because such preschool children cannot profit much from going into an educational situation. That is also why these same parents felt that day care should not be part of the public school system but rather a separate community agency.

A second type of task force should be an interdisciplinary, national group of researchers who are monitoring the directions research is taking, the problems that are being studied, and the gaps that exist. I realize that in this period of knowledge explosions this is a difficult task, but a lack of coordinated efforts and information sources is obvious. I recall vividly that at a meeting a representative of a particular foundation was making a plea for a certain type of research endeavor when someone mentioned that such was being done under the sponsorship of another governmental agency. Such a task force could be organized as a commission with a permanent staff that would be rotated in some prearranged way. I am aware of the interagency arrangements mentioned in the report here, but I would like to see this elaborated. I think the savings in avoiding duplications and redundancy might well pay for such an arrangement by furthering research. In addition, the commission would have as its charge making contact among investigators interested in the same problem or working in relevant areas. I want to make it clear, however, that I am not advocating programmatic research initiated by government agencies. Yet, I do not think that there is any set pattern here. Research can be initiated by government agencies, individuals, or any other source.

The larger issue, that needs to be dealt with by the third task force, is the criteria for support, the length of support, and the dissemination of research results. The guidelines followed by granting agencies and the question of accountability for dissemination and application of research results are crucial aspects of this issue. I see the need here for some social organization that will deal with these issues. There are vast amounts of material in the hands of government agencies that are disseminated by the discretion of the investigator. In the case of basic research within disciplines, there are controls for dissemination in professional journals arrived at through personal needs for recognition and university stress on publication for advancement. But the policy for disseminating knowledge to the relevant agencies is a different story; when these materials are distributed, they are arranged in a way most useful to other investigators, not policymakers. Why is there no continuity in federal planning? Is this something that can be changed, or is it indigenous to the system?

I do want to turn our attention to the role of professionals. Many of us operate from a physical model of investigation in which we evaluate our research in terms of its methodology and contribution to theory. Methodology also involves modes of analysis, and we are committed to a quantitative mode of expressing significance. The utility of this paradigm to the kinds of intervention research under discussion is a critical issue. Rather than refer to this issue as a paradigm shift, as is done in the report, I would like to think of this

as a relevance shift, a shift in the construction we have of the reality to which we are addressing our questions. For example, it seems to me impossible to replicate any of the social experiments referred to in this report. Replications are impossible because of constant change in social environments in which these studies have been done. Thus we are faced with assessing the utility of one-shot studies. Secondly, what are the significant findings? In large-scale social experiments such as Head Start what is considered significant? Do we quantify it in terms of numbers of children helped? Let us imagine an experiment in reading in which no statistical significance is found between treatment groups, yet we find that in actual numbers only five percent are helped. This five percent may involve thousands. Thus, if one is examining these findings from the statistical perspective, one would tend to discount the significance of the findings. On the other hand, if one eschews standard statistical procedures and works just with percentage figures of children who make gains or losses in such programs, one may have another interpretation. Many children may be affected by the program and hence, in absolute numbers, the results indicate some impact. To be sure there is always the possibility that the differences in scores between one time and another may be due to a host of errors (measurement, motivation of the children, etc.). Until we can employ more precise measurement and have confidence in our entire operation from the point of view of sound research practice, we are faced with the problem of stringency.

I now return to my basic point. We have a dual approach—using our wisdom and examining data from many perspectives to solve a social problem while simultaneously employing better research approaches. In this way we are dealing with the problems before us and also increasing our knowledge base. I would venture to say that the chances are that the crude measures and the sources of error in measurement and execution of research may not be too far apart. I suppose I believe that children are bound to make some constructive gains in our programs and that inspection of the results from an individual difference point of view will provide us with more empirical support than is currently the case with the use of analytic tools geared to probability models and to group data.

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And now we have the four-volume *Federal Programs for Young Children: Review and Recommendations.* In 1967, it was the report of the President's Task Force on Early Childhood Development, then the report of the Interagency Task Force on Children. There were the reports of the White House Conference on Children and of the Joint Commission on the Mental Health of Children. A report is due from the National Research Council's Advisory Committee on Child Development, and the report of the Carnegie Council on Children should be forthcoming. Offices of the federal government have commissioned reports (e.g., Bronfenbrenner 1974), groups convened by state governments have issued reports, and still other efforts on behalf of children have been supported by private foundations (e.g., McFadden 1972). Interested lay groups have published the results of their investigations and deliberations (e.g., Keyserling 1972).

All this activity during the past seven or eight years has been devoted to the same goal—developing public policy for programs dealing with young children. Highly competent groups have been assembled to discuss the issues and review the research. New studies have been commissioned. Opinions of a broad array of experts have been obtained. The number of pages of acquired evidence and argument has mounted into the thousands. But in reading these published reports, one is overcome by the feeling of *déja vu.* For the most part they say the same things: The needs of the nation's young children are not being met. Many young children are not getting adequate health care; they are not receiving a satisfactory diet; and they are not being exposed to the kinds of environmental stimulation that are believed to result in sound psychological development. Recommendations of all reports have common themes: delivery services must be improved, a broader network of child care facilities should be established, and parents should be involved in programs that intervene in children's lives.

There has not been a dearth of sensible recommendations emerging from these commissions and task forces. There has been repeated failure in translating these recommendations into action. Although a great deal of money—many hundreds of thousands of dollars—has been expended on commissions, committees, and councils, the early childhood scene has changed very little since the first report was issued. Budgets have not been increased; the number of children served remains more or less the same; there have been no notable innovations in health care or in services to children of disrupted or distressed families.

After finding that reports, year after year, come up with the same arguments and yield similar recommendations, one gains some understanding of the skepticism that is sometimes expressed over the utility of such reports. Critics may be correct in suggesting that action on a problem is best delayed by commissioning another report. Will this report produce a difference in federal

programs for children? I doubt it. And I doubt that the reports of the Advisory Committee on Child Development or the Carnegie Council will have any impact either, unless these groups face the painful question of how to create mechanisms that transform proposals into legislation. We do not need more reports. We do need to see that something is done about the reports that have been published or are in preparation.

Developing public policy and translating it into political action is not something with which persons in child development or early childhood education have had much experience. Only in the past few years—with the Surgeon General's Scientific Advisory Committee on Television and Social Behavior and legislation dealing with child care—have academicians and practitioners been confronted with questions that have important political implications. Ways must be found in which the child development specialist and the political activist can combine their talents to create legislation that will improve the lives of our children.

We need to look for help in political action, and we also need to examine more critically the kinds of recommendations that can be made on the basis of current research. The theme I would like to develop is that we need to reexamine the basis on which experts have been making recommendations concerning intervention programs for children. Presumably, the recommendations are derived from evaluations of various intervention projects. Writers of various reports have advised that caution be exerted in interpreting the results of some of the studies. As described in this report, the evidence is said to be "limited and argumentative." Nevertheless, the point is made that the recommendations have emerged from and been modified by the results of research. From my point of view, the published evidence offers not merely limited bases for making recommendations, it offers us *no* satisfactory basis for recommending one form of intervention over another, one program over another, or one curriculum over another. In my view, solid conclusions cannot be drawn from this research for the following reasons:

1. We do not know enough about the characteristics of the populations into whose lives we are intervening.
2. We know next to nothing about the manner in which the intervention was carried out.
3. We do not know much of anything about the consequences of intervention except as it influences the IQ score.

What do I mean when I refer to "characteristics" of the populations studied? My major concern is that we have been far too prone to consider disadvantaged children as deficient (a theme that has been developed by others in other contexts) and disadvantaged adults as retarded. This has been a procedural problem throughout the past 40 years of intervention research and, unbelievably, it

persists in studies today. We still are giving standard IQ tests and making judgments of retardation of mothers without sufficient basis for doing so. An example is the Heber project in Milwaukee, where all of the mothers were black. Why do we persist in giving IQ tests to individuals for whom the tests have never been standardized? How can we continue to characterize parents or children on the basis of inappropriate test scores? The influence of intervention can be interpreted only when we can specify with much greater precision the intellectual, social, and cultural characteristics of the individuals who have been subjected to the intervention. Conclusions about the effects of an independent variable must be qualified according to the types of individuals for whom the variable does and does not have an effect. It is doubtful that there is any form of intervention that will have equal effects for all children.

A second reason I believe we have made little progress in understanding the processes involved in intervention projects since the 1930s is that we do not know what goes on in intervention studies. Let us look at Levenstein's Verbal Interaction Project as an example. It is concluded that improvement in intellectual functioning in young children can be produced by verbal interaction between mother and child around cognitively challenging tasks. But we have absolutely no idea of what these mothers did with their children. We know that a toy demonstrator came to the home 30-odd times to show how mother and child could interact in play with interesting toys and books. We have no information about what the mothers then did with their children for no observations of the mother-child interaction were made. Solid conclusions about intervention can be made only when we know what was done. The published literature does not contain a single program in which there is careful documentation of what actually went on during the course of the program. There are bits of teacher-child conversations, guides for curricula, and lists of materials, but in no cases have extensive observations been made of the day-to-day course of the intervention.

Another example. We are told that structured curricula are better than traditional curricula, but never is an operational definition given of precisely what is done in executing the two curricula. If the cognitively oriented curriculum is shown to produce certain outcomes, is it because of a particular aspect of the curriculum, because teachers have had to spend more time in preparation, because the curriculum is presented more coherently? If the personal-social curriculum produces certain outcomes, is it because the children are more relaxed in the presence of adults, because they carry their lessons over into the concrete operations of play, because the teachers use more frequent social reinforcement? Until we know what was done—what the independent variables were—the studies must be considered loose demonstration studies, studies of the co-relation between the goals defined by the investigator and the measures taken at the end of the study, rather than studies in which attribution of effects can be made. To be convincing, causal rather than correlational statements are necessary.

A third reason why solid conclusions cannot be drawn from existing research concerns outcome measures. We know practically nothing about the outcomes of intervention, except as intervention influences the IQ score. Continued reliance on the IQ is unsupportable. The merit of IQ tests is that they yield scores that are objective and reliable, but professionals who have spent their lives working with young children have never looked at improvement in IQ scores as the measure by which they wish their accomplishments to be judged. Grade school teachers do not consider their year a success only if they raise their pupils' IQs. Yet change in IQ has been singled out in studies of intervention as the index of success. This is a misleading goal, and it is accompanied by several serious problems.

1. In many reports there is a failure to keep in mind that the results can be described only in terms of the IQ score. Though we would like to be dealing with intelligence, cognitive gains, or cognitive development, when we use the intelligence test as our only dependent measure, the IQ score is all we are talking about.

2. Here is a true paradox. Strong arguments have been made that the IQ is not a fixed index, but varies with the child's physiological and experiential status. In other words, the IQ is a labile characteristic. We are pleased when an intervention strategy results in increases in the IQ, but we are willing to condemn preschool programs, Head Start, or whatever when, upon the withdrawal of the positive experiences, the IQ goes down. We cannot have it both ways—labile when it goes up, but fixed after it is up. Does an improvement in mental age at one period in the child's life imply that the *rate* of mental development is increased forever after?

3. IQ tests were created to predict the child's potential for achieving in school. The IQ score in itself is of no great intrinsic value, but it becomes of interest as it increases our effectiveness in predicting other aspects of the child's behavior. What meaning do increases in IQ have, then, unless they are accompanied by changes in other characteristics of the child? Data seldom are available from intervention studies to answer this question.

4. The IQ is a composite score, representing the child's performance on an array of items requiring different types of ability. Are all these abilities equally affected by the intervention, or is there a differential effect on memory, vocabulary, general information, the ability to carry out instructions, or logical thinking—the abilities tapped on different items within the tests? The studies can be criticized not only for depending so heavily on IQ tests, but also for failing to present more exhaustive analyses of what even these tests reveal.

5. Finally, why do we continue to give IQ tests to children for whom the scores are invalid? We establish a standardization population and assume that the test scores of children can be compared to those of the standardization group

if the children have comparable language backgrounds, experiences, and motivation. Current tests were standardized on white, native-born, English-speaking children, and it is only for these children that we can offer a common interpretation of IQ scores.

Here, then, is my major criticism of the report. Despite what is said, recommendations of this report (and of all the other reports) cannot be derived from research data. What we have so far in the published literature on early childhood intervention are demonstration rather than research projects. The projects demonstrate the feasibility of various types of intervention but give no more than fragmentary and restricted pictures of the process and consequences of intervention. As a result, recommendations have been made on general knowledge about child development, experience with children, and common sense. The research on intervention has been pulled out to illustrate points writers wish to emphasize, rather than providing the objective basis on which the recommendations were made in the first place.

This could be an acceptable procedure, except for the fact that it places persons following such an approach in an extraordinarily vulnerable position. The use of inexact data to support one point of view permits other persons to use their inexact data to support alternative positions. Congressmen can be excused for accepting reports of large-scale evaluation projects that "prove" programs such as Head Start have only transitory effects and should not be supported, when earlier the expenditure of funds to initiate the programs was justified with equally flimsy evidence.

Why do we pretend to behave like scientists when, instead, we can behave like informed, decent men and women? We know a great deal about young children, and there are many knowledgeable persons in the nation capable of making wise comments about conditions that foster sound development in early childhood. Are such comments necessarily inappropriate bases for developing public policy? There is no scientific evidence that the expenditure of over $80 billion on defense will have the effects that this staggering amount of money is supposed to produce. Legislators have been willing to accept informed opinions in making such appropriations. Perhaps legislation will move along more rapidly if we were to rely—for the present—on informed opinion rather than inadequate research.

There is no reason why satisfactory research cannot be conducted. What will be needed are adequate funding (something that never has existed in this field) and the concentrated effort of some of the nation's ablest research talent. To accomplish this, federal agencies must realize that the best conditions for research do not exist when contracts are signed in September with final reports due in April, or when questions of the most profound complexity are presented for solution on budgets of $30,000 a year.

For now, there should be no more efforts to sort through bits and pieces of the intervention research. We can accomplish more if we seek means of getting

action on recommendations for which there is great consensus and if we begin to design research that is capable of giving richer answers to some of the questions we pose.

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Summarization of the Conference Proceedings

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Jules Richmond used to tell a story about a mother whale training her young pup to swim. After she had taught him all the movements, she reminded him that the thing that she now was about to teach him was the most important: "When you come to the top of the water to surface and spray or spout off, that's when you are most likely to get harpooned." In a sense we have put this report at the top of the water and it is being harpooned in that vulnerable position. In summarizing the four preceding commentaries and the subsequent discussion, it is clear that one of the things scholars are good at is criticizing each other. We criticized White's report rather severely. We may, however, have taken some of the printed words more seriously than they were intended.

There is a truer story that I also ought to tell before summarizing the proceedings. One of the elder statesmen of the field of educational measurement and educational research has said that he has been struggling with the task of trying to make sense out of the last 25 years of educational research. He had begun this task with a great deal of enthusiasm and had written his first several chapters and felt rather good. But when he approached the chapters where he was supposed to begin to distill the things that had been learned from educational research, he was thrown into a depression. He was frustrated and depressed because when he looked for definitive findings they simply were not there. He ended up deciding that perhaps in his later years he should become an educational philosopher rather than an educational researcher because it may be that with the complexities of human development, particularly if one considers them in the variety of social contexts in which they exist and develop, one has to be more reflective than quantitative, that the elegance of research may not speak as intelligently to these problems as we used to think. Here, most of us agree. White himself made the point that the strategies we depend on so heavily probably do tend to inform and probably do help us and give us some guidance, but they do not provide us with as clear answers as some of us used to think and as some of the people who turn to us for answers evidently still think.

Almost every comment has its qualifications or extensions or adaptations, but it appears that this report suffers in some ways from its lack of attention to the role of culture or cultures. When one looks at the problems of education or the problems of child development, attention is called to the fact that one of

the things that has been a terribly important influence on child development has been the changing role of women. We do not see much, if any, attention given to that social/cultural phenomenon against which some of the findings and the problems with which we are concerned will have to be evaluated.

Neither does the report look at the fact that the function of childhood has changed in our society. In an earlier period there was greater emphasis on bearing more children; they were an asset. In the current period we think of them in terms of their cost, and to many childbearing is thought of as a kind of consumer luxury. The shift from considering children as assets and childbearing as almost a moral responsibility to childbearing as a moral irresponsibility is a phenomenon that we need to be aware of and sensitive to. It has been suggested that these issues be included in our analysis of policy for children.

We are also reminded that the report tended to be evaluative or, if not value-free, its concern with values was not explicit. Yet if one reads the report carefully, the focus of helping youngsters catch up with the middle class is clear. The report very clearly supports individualization, taking seriously the needs of children and the many adaptations in meeting their needs. It seems to reflect the patience with or growing tolerance of pluralism as a phenomenon. Yet these values are not made explicit and these values do influence what happens and the effectiveness of what happens. Many also feel that there is a problem in the report, as well as in the field, that relates to the paradox of adaptation: the fact that as youngsters develop and respond to their environments, they also influence their environments. And what is even more important and was much discussed is the fact that even though we are reasonably well-convinced that youngsters are uniquely adaptive and that this capacity for adaptation is one of the unique characteristics of human organisms, the things that we have done to deliberately guide that adaptation, the things we have done to try to educate and to shape development, seem to be relatively ineffective.

It is not clear though that we have been as ineffective in achieving the purposes of education as was argued. Given the purposes served by education, we may have been relatively effective in achieving the things that society *wants* of education—a point to be discussed later. It may be that society is not as seriously committed to some of the goals of education that we espouse "for" society. The problem may be that we are not ineffective in doing what society really wants, but we are ineffective in doing the things that logic and good sense and decency tell us that society ought to be wanting.

At another point in our discussion, it was suggested that by looking at the kinds of inputs, the kinds of treatment that are available to youngsters through the programs that we have studied here, we may be examining a phenomenon that is at the periphery of the developmental process rather than at the center of it. Put in a different way, it may be that education and our formal intervention in the development of youngsters may not work in sufficiently crucial and critical ways for the intervention to result in the outcomes that we think are important. We criticized the lack of attention to politics and economics in the

report, but that research tends to be apolitical, acultural, and developed with little attention to the social contexts in which the treatments occur. And we repeatedly found ourselves saying that in the absence of a serious concern for the context in which these things occur (what I like to call the ecology of human development or even the ecology of institutional development), we may be dealing with an insufficiently broad range of the input variables. So if one is not studying the political economy of education at the same time with technological problems of education or the delivery of educational interventions, one may not have a broad enough range of the variables that are involved in education to accurately claim to have taken into account the important variables.

Now despite the criticisms of this sort, there were many who felt that White and his associates had done a useful job; that is, the variety of studies he plowed through, tried to understand, and drayed insights from were useful in helping us to see where we are with respect to the state of the art in research and evaluation technology and with respect to the insights that we can draw from this body of data. But it also led us to suggest that if we are to find meaningful answers to the questions that really trouble us about childhood, child development, and education, we will need to go beyond these kinds of studies.

There is a need for assessment of processes with an ecological approach. It has been mentioned that some feel we are tinkering with peripheral institutions; that is, education on the level of preschool, elementary, or secondary may not be as essential to the processes of development as we think. It may not be as important as some other aspects of the life experience. Attention was drawn to the fact that data gathering is important but mentation—thinking about those data—is even more important; talk about the data cannot be substituted for talk about what the data *means* and its implications—what the consequences are for institutions, individuals, and society and the consequences that flow from knowledge applied in certain ways. Do we miss the forest through the trees?

Frequently we have been reminded that we have an even greater responsibility to use our wisdom when applying the findings of science and that the application of quantitative data simply is not enough. In fact, it has been suggested that given our impressions of the limitations of our fields, an alternative strategy may be to bring together informed, decent, intelligent people to share their experiences and wisdom in making judgments that lead to policy decisions. I do not believe that this alternative strategy can be offered as a consensus, but there may be some of us who think, however, that this strategy might be more productive than the pursuit of research, given the present status of research in the behavioral and social sciences. Other members of the group may feel that there is still room for, and certainly hope that, evaluative research and other research strategies can deal intelligently with policy issues. What has been suggested repeatedly during these meetings and is probably one of the best strategies at the moment is to consider more systematically the knowledge and information we have available about the problems which confront us and to depend less on the researcher or the evaluator to give us "answers" to policy questions.

We have agreed that to expect any study to supply the definitive answer is wrong. We have also agreed that the expectation is based upon mistaken assumptions. In this connection we covered some of the functions and problems involved in evaluation and research and agreed that, in the absence of more refined research and evaluation technology, there is something to be gained from the examination of many studies that deal with the same problem but in different ways and with different variables. When we begin to find trends and congruencies in these data, even though these studies have their weaknesses, we may be reasonably safe in drawing inferences from them. Yet some are still uncomfortable with that solution—feeling that if the studies were not good, then the data is not good, and therefore anything drawn from them may be inappropriate or weak. I am reminded of Hebb's reference to a colleague who asserted, "Anything that's not worth doing, is not worth doing well." Some of us might argue that if you have lousy studies, then there is no point in gathering them up and trying to make sense out of them. But there may be others who feel that trends apparent in a large body of research on the same issues may be useful in making policy decisions.

Some attention was given to the apparent ineffectiveness of research in these areas and to the fact that we are not yet able to definitely associate specific treatments with specific outcomes. Some view it as tragic that so much money and time have been spent on studies like those reviewed here only to find that they tell us relatively little. Others are less pessimistic and feel that all of the work has not been for naught. The investments of effort and money do reflect professional and national concern. The level of questions posed for study has been raised, interesting leads have been uncovered, and some new promising directions have been identified. We are better prepared to study some of the problems today than we were 10 years ago. For example, in discussing the weaknesses of this research, our attention has been repeatedly called to the fact that much of this research focused on the IQ or IQ-related outcomes when there are many other outcomes to be expected from the treatments that are involved in these many studies. A part of the problem may be that we have been insufficiently sensitive to the wide variations in outcomes and unappreciative of some of the outcomes simply because we are not looking for them or do not know how to measure them.

It was suggested that we have been harmed by a mechanistic approach to research. With regard to the affective domain, we have developed rather elegant strategies for experimental research and have insisted on following only those strategies. Similarly, we may be caught up in the validative tradition of research, which has caused us to be inattentive to the many categories of variables and relationships that could be studied; the strategies traditionally utilized in validative research (hypothesis testing) have not allowed for attention to dialectical relations between variables. In order to test our hypotheses we must impose the kind of controls and rigorous conditions necessary for validation, yet in sterilizing the conditions and isolating the target variables we

may exclude or ignore the very sources through which understanding might be possible. It may be that what should be examined are not unitary and isolated dependent and independent variables, but clusters and patterns of variables in continuous dialectic interaction. My colleague, Esposito, and I have referred to this as the study of the dynamic blending of variables. The study of such blending cannot be dependent upon regression analysis in which the cumulative impact of multiple variables is progressively summed. We must seek procedures which elucidate the mechanisms by which multiple variables function to produce differential effects.

The problem here is that we simply do not know how to do that kind of research. We do not know how to study such complex social phenomena where so many variables are operating, where a single variable may in one instance be a dependent variable and in another an independent variable. Since the position is constantly changing, we have tended to try to pull out *the* variable, the crucial variable, or look for the relationship between a couple of the variables. Even in our more sophisticated research, where we try to ascribe weights to multiple variables through regression analysis, those regression analyses tend to produce arithmetic progressions when the reality of the relationships being studied probably involves algebraic progressions. We have not developed strategies for studying those interactions in ways that reflect what may be the true nature of the relationships.

Thus the things we have to study may be far more complex than the strategies that are currently available. And this kind of research is terribly costly. Someone estimated that many of the studies included in White's report cost less than $50,000. Obviously, some of them cost several hundred thousand dollars, but the point being made was that the money available for evaluation and research has often been too little or was applied in insufficient concentration to enable good work. This issue was debated. Some claimed that the amount of money is not the problem but that conceptualizational technology is the problem. Others have argued that there is a negative correlation between increased costs and quality of the research. We were also reminded that we have problems related to the nature of the commitment to evaluative research and the quality of people available to conduct it. My own position draws on all of these arguments. There clearly are some questions that can be studied intelligently with very limited funds. Some good research was obviously conducted before any of us even dreamed of the $500,000—and $1,000,000—research grant. There can be no question about the importance of commitment and the importance of power and technology applied to these problems. But if we are talking about action research—i.e., evaluation research in live settings, where we take into account the ecology of the phenomena as well as the dynamic blending of variables—we are not talking about low-cost, overnight studies. The kind of serious work that is implicitly and explicitly called for will cost money.

Some of the specific recommendations of this report were discussed during the conference. For example, it was recommended that preschool programs be

limited to the handicapped and disadvantaged children. It appears that this recommendation grew out of the concern for cost efficiency. One of the commentators suggested that this recommendation assumed that those who specialize in special education know what it is all about. In other words, he was suggesting that this field and its technology may be no better than the technology of any other area of education and, therefore, recommending concentration on any area may be inappropriate without an examination of the quality of developments.

Another problem related to this recommendation, understandable as its fiscal rationale may be, is its possible contribution to the further segregation and stigmatization of handicapped, low-income, and minority-group children. One of the disadvantages of the Head Start and Title I efforts is that by law these programs must serve only or primarily the target populations, and in serving them, the programs tend to isolate and stigmatize the participants. Yet a strategy that focuses on all children is likely to result in the neglect of the special concerns of those most in need.

The report also suggests that structured programs appear more effective than unstructured programs. This is an observation that many have asserted, but most of us will acknowledge that there is no definitive evidence on the subject. Even more important is that there may be other variables involved in structured treatments than just the fact of structure. In other words, if the program is highly structured, it may be that teachers spend more time in preparing for its delivery, or they understand it more, or that something else is associated with the fact that it is more structured. The recommendation sounds colloquially good, but one must be reminded that it is a recommendation dictated by the data.

These criticisms of the report's recommendations were made before the author had a chance to address the group and to put these recommendations into context. Upon doing so, he called our attention to the fact that after he had reviewed the existing data, he found they were not as directly helpful as anticipated. He actually had to take the leap that some have suggested is essential to policymaking in this field. Taking the knowledge available, personal experience, the best available data, and applying some solid thought and judgment, he hoped to arrive at the wisdom upon which to draw conclusions and make policy recommendations. In effect then, White said that in the construction of the report, although the recommendations look like they grew out of the data, they actually grew out of experience in dealing with the data and, more importantly, his *thought* about the data and his experiences. It was never claimed that these recommendations are justified solely on the basis of the report's data.

The recommendation calling for greater attention to individualization was also criticized. While we are increasingly aware of individual differences, our treatment methodologies have not been varied sufficiently to deal even with existing knowledge. It appears that greater attention needs to be given to individual differences and to the fact that we do not have sufficient treatment

variants. Possibly, it is time to 1) examine individual differences more fully, 2) understand them better, and, hopefully, 3) generate ways of designing more appropriate treatments.

In our discussions, we attacked but never resolved the controversy of whether definitive evaluations leading to policy decisions can actually be made. Some feel that programs can be studied in such a way as to inform policy. Most would agree that if the informing process must be viewed as definitive, as completely authentic, we are nowhere near being capable of delivering those kinds of answers. But there are informed leads, some good hunches, and some wise advice that can be provided on the basis of existing evaluation and research data. In this connection, I am reminded again of Hebb who talks about the function of theory. He suggests that theories are not to be accepted as the final word cast in concrete, expected to exist forever. Theory is simply to be used. Theory gives one a handle for tackling a problem. It provides a framework with which to view a problem. And for the good practitioner, and certainly the good researcher, we are always concerned with disproving the theory, or reformulating it and raising it to a higher level of conceptualization.

In terms of existing evaluation and research strategies for informing policy, if one takes our findings, our conclusions, and our advice as tentative, as guides for current practices but not necessarily as guides for eternal practices, this is a reasonable expectation and use of our work. That is, we can suggest where we are and what our best bets are at the moment, but the process of knowing is a continuously evolving process, and where we will be next week or next year or five years from now is likely to be different from where we are at the present time.

The last point to be made on the subject comes from a note I made for myself as I was listening to the discussion. We sometimes forget, when we are looking for tools for research and evaluation, that the brain is also a research tool. I have not associated the previous comments with individuals, but this is one point that Urie Bronfenbrenner made so frequently that I have to identify it with him. He repeatedly reminds us that the fanciest research and evaluation strategies and the best data can only take us so far. In the final analysis, we are professionals and researchers and must apply some brain power, informed judgment, and common sense in order to understand and to arrive at policy recommendations.

One final reservation cited was the fact that it proceeds from a base of sheer rhetoric with respect to what school is all about. And that rhetoric may not correspond to the reality of what schools are today. It may be that schooling involves many more things than the ways in which it is commonly perceived. It certainly is in part related to the money spent, or the kind of equipment that is available, the nature of the curriculum, the nature of the teacher, the nature of teacher-pupil interactions—all those in-school things. There are, in addition, out-of-school phenomena which also must be considered. There is the matter of what the experience represents to the people who are involved in it, which

may be different from what it represents to the people who decide to sponsor it, or the people who deliver services to it, or those of us who observe and study it. Then, finally, there are social contexts in which education and developmental experiences occur. What are the purposes for which these services, these experiences, are made available to the children? It is possible that these contextual variables may be more important, may be more powerful determinants of the effectiveness of schooling, than are the formal inputs.

An anthropologist, Anthony Wallace, has written about the functions of schooling in societies in different phases of development. He defined these societal phases as revolutionary, conservative, and reactionary and defined the functions of schooling as relating to the development of intellect, morality, and of skills or techniques. He suggested that depending on the phase through which the society is passing, the priority given to these functions shifts. A society in its revolutionary phase may give greater attention to the development of morality (human values and opportunities) and intellect (knowledge and understanding) and neglect the development of skills (technical know-how). One may read purpose into these different emphases, but my point in using the reference is to carry the conceptions beyond the point where Wallace developed them. To be maximally effective, one would anticipate congruence between the societal purposes of education and the learner's purposes. But learners of certain subgroups may not be in the same phase of development as the mainstream of the society. Consequently, priorities with respect to the functions of education may differ not only between societies but for groups within a society. If the purpose of schooling is to train people in skill development and that is their purpose for attending school, it is likely that schools will do a reasonably effective job. But if the recipients of that service are more concerned with moral development or intellectual development, the fact that the recipients have one set of values and goals with respect to schooling and the society has another set brings about a kind of conflict that will interfere with the effectiveness of the process itself. There must be a congruence between the purposes that society assigns to education and the purposes which have meaning for the persons who are the beneficiaries of that education.

We began here by calling attention to the fact that this report gave insufficient attention to the cultural, economic, political, and social contexts in which child development and education exist in this country. We discussed the limitations of evaluation and research technology which prevent us from effectively informing public policy based on these data. We criticized and pointed to a few directions in which new work might begin. We stressed the importance of utilizing thought and judgment as instruments of understanding. And we come back to the point where we began. To try to understand phenomena independent of their situational and purposive contexts results in limited understanding and may result in futile effort.

Remarks Concerning the Conference Proceedings

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I want to address three different points that were brought to mind by the report and our discussions of it.

1. Some factors which I think, and the people we work with in policymaking seem to think, are affecting child development policy were not discussed in the report and do need our attention.
2. Some general questions having policy implications are raised by the content and methodology of the report and require further examination.
3. Some specific practical implementation questions being asked by policymakers could perhaps be discussed at greater length at this kind of a forum.

We have spent a great deal of time criticizing this four-volume report. I am sure a lot of the criticism is justified, but I also think that it really is a very important contribution. There is a great deal of information in this report that people who make policy decisions would like to get their hands on. The health sections, for example, might be particularly useful. I would be interested in knowing more about the HEW policy of distributing this report. Who has received it? Who is going to receive it?

There are several major factors that I think really do influence the kinds of policy that are being made in child development across the country. The first is that although this report was directed toward federal programs and federal planning, I think the action really is going to be in the states. It will not be in the federal government for a variety of reasons. Some of you can put your finger on such things as revenue sharing, decentralization of HEW, the unlikely prospect, at least this year, of any new federal legislation. Related to this is a concern in the states and in the Office of Child Development as to how services should be delivered in the states. The whole question of the mechanism for planning and coordinating programs will directly affect these programs. What, for example, should be the role of the schools? Are we justified in setting up delivery mechanisms which, as they tend to be doing, bypass the schools? As you know, some 17 states have set up state offices of child development, most of which are not in state departments of education. Some cities have the equivalent. While this is happening, there are, however, no agreed upon

criteria for measuring the capacity of states or any other unit of government to deliver these services. The Office of Child Development is interested in this problem, but at the moment we do not have criteria for measuring capacity or for planning methods to build that capacity. I think that this kind of issue is related to the discussions that have and will take place in Congress about prime sponsorship—the role of the states versus the role of local governments as to whom should control the purse strings.

This, of course, brings us back to the problem of how the history of child development funding impacts upon delivery systems. The problem of coordination at the state level is made much more difficult because the states have to respond, and have responded in the past, to the various categorical kinds of funding that Dr. White discussed. If there are more than 200 federal programs, you have to do something about cleaning up the number of different programs and the number of agencies which fund them if you can really expect any other unit of government to be able to coordinate and plan.

Another emerging trend is an interest on the part of the OCD to revise its research policy toward more practical ends. If that is in fact true, it is a very opportune time for a group like this to talk about the form that national research policy in child development might take. There is, of course, a simultaneous trend in HEW towards systems analysis and sytems management. We are hearing many complaints from people who deal with HEW as to their overreliance on systems analysis people.

Lastly, it is very easy in a group like this, and perhaps even in writing a report like this, to assume that there are many people around the country who really believe in child development and that there exists a constituency for child development. Perhaps I do not need to inform you that this really is not the case. The strongest unified group that we hear from are people who are concerned about day care for very immediate reasons; this is related, of course, to the changing role of women that William Kessen previously discussed. Some of the factors determining the effectiveness of programs are evident here. It is difficult for any professionals, including developmental psychologists, to agree upon priorities. There is a very real concern among people who deliver and administer programs at the state or local level about protection of their area of responsibility, fiscal appropriations, and, therefore, who will be able to continue to function.

I think that there is no outcry around this country for child development services per se. Consumers, parents, and families are not demanding comprehensive child development services in any unified fashion that would affect political decisionmaking. There are people who care about day care and there are parents of the handicapped who want different kinds of special education or parent training programs. For us to sit and talk about how child development is wonderful and how people ought to proceed is very impractical; there are no people out there who are going to put pressure on political decisionmakers to bring it about.

If there is a cry out there, it is inarticulate because nobody really knows what the cry is about. I asked Dr. White for his definition of general preschools. I could say everything he said about general preschools and about child development. Of course everybody uses the term child development and I am sure no one thinks of it exactly in the same way. When I talk about child development, I think of all kinds of justifications for it. Yet, I do not believe there is much justification for White's conception of general preschools because I think, whether we like it or not, it is cast in people's minds in terms of a continuation of school. Perhaps this is not the view held by most child psychologists, but to most people school is thought of as four walls with kids in groups and preschool is the lower echelon of this structure.

There are at least three general questions that have been raised by the report and the discussion of it that have important policy implications. The first relates to present research methodology and evaluation techniques and what they do or do not indicate about program priorities. I believe the developmental psychologists are saying that the indications from research methodology and evaluation techniques for program priorities are very limited. Therefore, one makes a leap in space that is very convincing until you are in the position of a state legislator or a federal legislator or a state agency administrator having to make a hard decision about whether to spend money on day care or on early screening or, perhaps, on a new community college. Policymakers have a very strong tendency to ask for "go" or "no-go" kinds of information. Just saying that our evaluation techniques are inadequate to provide that information will not solve the problems. I think we need to face this problem and either try to reeducate decisionmakers or develop new techniques.

This relates to the second question concerning cost-benefit data that we discussed previously. Dr. White seemed to reject cost-benefit analysis either because of lack of data or because he found no justification for its use. But policymakers think in these terms; they want answers. It seems to me that it is essential for us to consider whether cost-benefit analysis can be done, whether it should be done, and when it is not feasible, what policymakers should be told to replace it with.

Thirdly, I am still not satisfied with White's answer to the long-term research policy question. I have reviewed volume three and the questions raised there are very good, but it seems to me that we need more discussion of this particular question. We probably do need a long-term, intramural nongovernmental group. There certainly is a feeling among people who allocate funds that there has already been a great deal of research, that we ought to be able to draw conclusions from what has been done, and that we should not put more money into new demonstration efforts, thereby throwing good money after bad. We need to come to some consensus, if it is at all possible, on what the priorities should be in the case of a long-term research policy relating to child development.

As to specific practical implementation questions coming out of the report and asked about by policymakers, they are many and varied. They are "how to" questions related to credentialing, day care licensing, needs assessment and planning, techniques of local control and involvement, methods of designing financing packages to fund all these programs that people say they want, the kinds of staff training necessary, and so forth. We ought to follow up with people who are more directly involved in policymaking or issues such as day care. I think people who read the report will say that nonharmful day care, whatever that really is, is probably a good investment, or at least if there is sufficient pressure for it, then a state would be justified in launching that kind of program. I know state OCD directors facing that specific decision. Should they go ahead and recommend that a broad-scale day care program be implemented? The issues raised in White's recommendations about kindergarten through grade three schooling are very intriguing. Perhaps we know so little about what happens before kindergarten that we might be justified in simply trying to reform the kindergarten through grade three level and see what that does. This a very intriguing thought to many policymakers. Perhaps it is better to try to reform what you have rather than just extend it downward. Shouldn't we look at that?

We also discussed ecological intervention. Is income maintenance more important, for example, than putting those funds into a day care program, or is that kind of approach just a red herring? Can we come to any conclusions about the broader approach versus the more specific ones? It would be interesting to follow up on the question of family support or strengthening the family versus interventionism. I believe we were really getting into some of those issues about how it can be done and whether it should be done.

Basically, policy people and program people are asking the developmental psychologists what there is a consensus on. It is easier to sell programs if there is a consensus. Nothing was more of a treat to policymakers than Bloom's notion that most of the child's intelligence is developed in early years. That was irrefutable evidence, or at least that is what they were told.

Policy people could deal with this. It is a question of saleability. There are other ways that policy is sold. Some is through public pressure; we have discussed this in relation to day care. Another way that policy is sold is what we call, for lack of a better term, a "sexy" issue. Child abuse is a sexy issue; you don't have to have a constituency out there to do something about child abuse because you get lots of publicity by taking action. But it comes back to what is the consensus, what really is known, what issues are saleable? How do we sell child care?

EPILOGUE

We have chosen to end our discussion as an epilogue rather than as a summary or collection of statements. The material that we have presented is rather diverse. Half of it is devoted to the process of establishing a rational dialogue between public policymakers interested in the problems of child care and those who study the problems of child development. The other material concerns the content around which our attempt at dialogue took place. It would not be possible to summarize the year's work without repeating much of what we have already said. We feel our effort and the efforts of others who attended the conferences, read the report, wrote critiques, and thought about the problems of child care and public policy was not wasted. We came to the end of our effort with as many if not more questions than when we started. We have not resolved these questions. We have merely begun to make the first effort toward a complex social, political, and intellectual interaction. Other attempts on different levels and for different reasons need to be made. We can only hope that our experience and our formal presentation of that experience will help others in making further efforts.

We believe that exploration should now take the form of an active engagement of the study of this problem through experimentation around selective scenes. Philosophical discourse on the role of systems, public policy, and child care, although valuable and important, should now give way to an active attempt to provide a rational process for public policy on the care and growth of our children.